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A

MONTHLY REVIEW OF ASTRONOMY.

EDITED BY

H. H. TURNER, M.A., B.Sc., Sec. R.A.S.,
AND
A. A. COMMON, LL.D., F.R.S., Treas. R.A.S.

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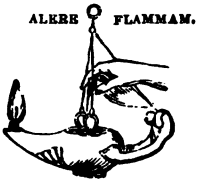
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ANNUAL COMPANION

TO

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 183. 1892. Vol. XV.

INTRODUCTION.

The present 'Companion' closely resembles that of last year.

As in former years, we have to thank Mr. Denning for revising the "Meteor Notes," and Mr. Lewis for the Double Stars, for a number of which Mr. Maw has kindly supplied observations. M. Loewy has again favoured us with advance-proofs from which we have obtained the Variable Star Ephemerides. The occultations and phenomena of Jupiter's Satellites are from the 'Nautical Almanac'; the diagrams and Ephemerides of the Satellites of the other planets from the 'American Ephemeris.'

The "Fraction of the Year" is the fraction which has elapsed at mean noon of the particular day. The Moon's declination is given for mean midnight. The "Longitude of the Moon's Terminator" is given for mean midnight; the letters M. and E. signifying morning and evening—that is, that the Sun is rising or setting on that particular longitude of the Moon's surface; the sign – indicates longitudes reckoned from the central meridian in a westward direction. The angles of disappearance and reappearance for occultations are reckoned from the *true* north in the direction N. E. S. W., as for double stars.

Greenwich Mean Time is used in all cases, and the astronomical day is reckoned from noon to noon as in previous years.

Date.	Day of Year.	Fr. of Year.	Sun		☉'s Dec. Noon.	M.T. at Apparent Noon.	Sid. T. at Mean Noon.	Moon's Phases.
			Sets.	Rises.				
			h m	h m	° ′	h m s	h m s	d h m
Jan. 3	3	·006	4 1	20 8	22 51 S	0 4 34	18 50 20	☽ 6 13 12
10	10	·025	4 10	20 6	22 0	7 38	19 17 56	○13 15 27
17	17	·044	4 21	19 58	20 48	10 13	19 45 32	☾21 15 43
24	24	·063	4 33	19 51	19 16	12 14	20 13 8	●29 4 39
31	31	·082	4 46	19 41	17 27	13 38	20 40 44	
Feb. 7	38	·101	4 57	19 29	15 23	14 21	21 8 19	☽ 4 21 39
14	45	·121	5 10	19 16	13 6	14 24	21 35 55	○12 7 38
21	52	·140	5 23	19 3	10 38	13 51	22 3 31	☾20 12 15
28	59	·159	5 36	18 48	8 3	12 47	22 31 7	●27 15 47
Mar. 6	66	·178	5 48	18 34	5 22	11 17	23 58 43	☽ 5 7 15
13	73	·197	6 0	18 18	2 37 S	9 27	23 26 19	○13 9 55
20	80	·216	6 12	18 1	0 9 N	7 24	23 53 55	☾21 5 16
27	87	·236	6 24	17 45	2 54	5 16	0 21 31	●28 1 18
April 3	94	·255	6 37	17 29	5 36	3 9	0 49 7	☽ 3 18 21
10	101	·274	6 46	17 13	8 14	0 1 10	1 16 42	○11 18 26
17	108	·293	6 59	16 58	10 44	23 59 24	1 44 18	☾19 18 0
24	115	·312	7 11	16 45	13 7	57 58	2 11 54	●26 9 47
May 1	122	·331	7 23	16 31	15 18	56 56	2 39 30	☽ 3 7 12
8	129	·350	7 33	16 22	17 18	56 19	3 7 6	○11 10 59
15	136	·370	7 44	16 11	19 3	56 10	3 34 42	☾19 2 53
22	143	·389	7 53	16 2	20 32	56 29	4 2 18	●25 17 49
29	150	·408	8 2	15 52	21 44	57 15	4 29 54	
June 5	157	·427	8 9	15 48	22 38	58 21	4 57 30	☽ 1 21 51
12	164	·446	8 15	15 45	23 12	23 59 41	5 25 5	○10 1 32
19	171	·465	8 18	15 44	23 27	0 1 11	5 52 41	☾17 9 1
26	178	·485	8 18	15 46	23 21	2 41	6 20 17	●24 2 7
July 3	185	·504	8 16	15 52	22 55	4 4	6 47 53	☽ 1 14 13
10	192	·523	8 13	15 59	22 10	5 10	7 15 29	○ 9 13 44
17	199	·542	8 6	16 6	21 6	5 56	7 43 5	☾16 13 48
24	206	·561	7 56	16 15	19 44	6 16	8 10 41	●23 11 31
31	213	·580	7 46	16 27	18 6	6 8	8 38 17	☽31 7 45
Aug. 7	220	·600	7 34	16 37	16 14	0 5 29	9 5 53	○ 7 23 57
14	227	·619	7 21	16 47	14 9	4 22	9 33 29	☾14 18 37
21	234	·638	7 7	16 59	11 53	2 50	10 1 4	●21 22 59
28	241	·657	6 53	17 10	9 28	0 0 56	10 28 40	☽30 1 29
Sept. 4	248	·676	6 37	17 21	6 55	23 58 43	10 56 16	○ 6 9 8
11	255	·695	6 20	17 32	4 17	56 20	11 23 52	☾13 0 50
18	262	·715	6 5	17 43	1 35 N	53 52	11 51 28	●20 13 16
25	269	·734	5 50	17 55	1 8 S	51 26	12 19 4	☽28 18 19
Oct. 2	276	·753	5 35	18 7	3 52	49 9	12 46 40	○ 5 18 12
9	283	·772	5 18	18 19	6 33	47 7	13 14 16	☾12 9 37
16	290	·791	5 2	18 30	9 10	45 28	13 41 51	●20 6 24
23	297	·810	4 47	18 42	11 41	44 19	14 9 27	☽28 9 26
30	304	·830	4 34	18 55	14 2	43 44	14 37 3	
Nov. 6	311	·849	4 23	19 7	16 13	43 46	15 4 39	○ 4 3 49
13	318	·868	4 12	19 19	18 11	44 29	15 32 15	☾10 22 2
20	325	·887	4 4	19 31	19 53	45 55	15 59 51	●19 1 19
27	332	·906	3 56	19 42	21 17	47 59	16 27 27	☽26 22 28
Dec. 4	339	·925	3 51	19 52	22 21	50 38	16 55 3	○ 3 14 17
11	346	·945	3 49	20 0	23 5	53 44	17 22 39	☾10 14 30
18	353	·964	3 50	20 6	23 25	23 57 7	17 50 15	●18 20 13
25	360	·983	3 53	20 8	23 23	0 0 37	18 17 50	☽26 9 22

For Moon's phases: ● signifies New; ☽ First Quarter; ○Full; ☾ Last Quarter.

JANUARY.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	5 10	1 24	23 44 S	−62·3 M.
2	S	6 33	2 22	20 2	−50·1 M.
3	**S**	7 58	3 16	15 9	−38·0 M.
4	M	9 26	4 8	9 26	−25·8 M.
5	Tu	10 49	4 58	3 14 S	−13·6 M.
6	W	12 12	5 46	3 7 N	− 1·4 M.
7	Th	13 35	6 34	9 17	+10·7 M.
8	F	14 59	7 24	14 58	+22·9 M.
9	S	16 24	8 17	19 50	+35·1 M.
10	**S**	rises	9 11	23 34	+47·2 M.
11	M	1 29	10 9	25 54	+59·3 M.
12	Tu	2 14	11 7	26 42	+71·5 M.
13	W	3 14	12 5	25 55	......
14	Th	4 25	12 59	23 44	−84·2 E.
15	F	5 40	13 49	20 25	−72·1 E.
16	S	6 55	14 37	16 13	−59·9 E.
17	**S**	8 7	15 20	11 26	−47·8 E.
18	M	9 18	16 1	6 18	−35·6 E.
19	Tu	10 26	16 41	1 0 N	−23·4 E.
20	W	11 34	17 20	4 19 S	−11·3 E.
21	Th	12 44	18 0	9 29	+ 0·9 E.
22	F	13 54	18 43	14 22	+13·1 E.
23	S	15 8	19 29	18 45	+25·2 E.
24	**S**	16 22	20 18	22 26	+37·4 E.
25	M	17 36	21 12	25 9	+49·6 E.
26	Tu	18 42	22 10	26 35	+61·7 E.
27	W	19 36	23 9	26 30	+73·8 E.
28	Th	sets	......	24 47	......
29	F	4 8	0 8	21 27	−81·9 M.
30	S	5 36	1 6	16 46	−69·7 M.
31	**S**	7 2	2 0	11 5 S	−57·5 M.

FEBRUARY.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	8 31	2 52	4 46 S	−45·3 M.
2	Tu	9 59	3 42	1 46 N	−33·2 M.
3	W	11 25	4 32	8 8	−21·0 M.
4	Th	12 47	5 22	14 0	− 8·8 M.
5	F	14 12	6 13	19 4	+ 3·4 M.
6	S	15 34	7 7	23 1	+15·5 M.
7	**S**	16 52	8 3	25 38	+27·7 M.
8	M	17 54	9 0	26 46	+39·8 M.
9	Tu	18 45	9 57	26 23	+52·0 M.
10	W	19 19	10 51	24 34	+64·1 M.
11	Th	rises	11 43	21 33	+76·2 M.
12	F	4 37	12 30	17 35	......
13	S	5 51	13 15	12 56	−79·4 E.
14	**S**	7 3	13 57	7 50	−67·2 E.
15	M	8 12	14 37	2 31 N	−55·0 E.
16	Tu	9 20	15 16	2 51 S	−42·9 E.
17	W	10 30	15 56	8 6	−30·7 E.
18	Th	11 38	16 37	13 5	−18·6 E.
19	F	12 51	17 21	17 36	− 6·4 E.
20	S	14 5	18 8	21 30	+ 5·8 E.
21	**S**	15 17	18 58	24 31	+18·0 E.
22	M	16 26	19 54	26 25	+30·1 E.
23	Tu	17 24	20 51	26 55	+42·3 E.
24	W	18 11	21 50	25 51	+54·5 E.
25	Th	18 45	22 48	23 10	+66·6 E.
26	F	19 11	23 45	18 58	+78·8 E.
27	S	sets	......	13 32	......
28	**S**	6 2	0 39	7 13	−76·8 M.
29	M	7 33	1 31	0 28 S	−64·6 M.

MARCH.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	9 1	2 23	6 17 N	−52·5 M.
2	W	10 29	3 14	12 35	−40·3 M.
3	Th	11 57	4 7	18 5	−28·2 M.
4	F	13 23	5 2	22 26	−16·0 M.
5	S	14 43	5 58	25 25	− 3·8 M.
6	**S**	15 51	6 55	26 53	+ 8·3 M.
7	M	16 45	7 52	26 48	+20·5 M.
8	Tu	17 24	8 47	25 17	+32·6 M.
9	W	17 52	9 39	22 31	+44·8 M.
10	Th	18 13	10 27	18 46	+57·0 M.
11	F	18 29	11 12	14 16	+69·2 M.
12	S	rises	11 55	9 16	+81·4 M.
13	**S**	6 0	12 35	3 57 N	......
14	M	7 9	13 14	1 27 S	−74·2 E.
15	Tu	8 18	13 54	6 47	−62·0 E.
16	W	9 28	14 35	11 53	−49·8 E.
17	Th	10 38	15 17	16 35	−37·7 E.
18	F	11 52	16 2	20 40	−25·5 E.
19	S	13 2	16 51	23 56	−13·3 E.
20	**S**	14 12	17 43	26 10	− 1·2 E.
21	M	15 15	18 38	27 9	+11·0 E.
22	Tu	16 5	19 35	26 40	+23·1 E.
23	W	16 43	20 32	24 39	+35·3 E.
24	Th	17 12	21 28	21 6	+47·4 E.
25	F	17 32	22 22	16 12	+59·6 E.
26	S	17 50	23 15	10 14	+71·8 E.
27	**S**	sets	......	3 33 S	+84·0 E.
28	M	6 28	0 8	3 24 N	......
29	Tu	7 57	1 0	10 10	−71·5 M.
30	W	9 32	1 54	16 15	−59·3 M.
31	Th	11 0	2 50	21 16 N	−47·1 M.

APRIL.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	12 28	3 48	24 51 N	−35·0 M.
2	S	13 42	4 47	26 50	−22·8 M.
3	**S**	14 43	5 46	27 10	−10·6 M.
4	M	15 28	6 43	25 57	+ 1·6 M.
5	Tu	15 58	7 36	23 26	+13·8 M.
6	W	16 23	8 26	19 52	+26·0 M.
7	Th	16 37	9 11	15 30	+38·2 M.
8	F	16 52	9 54	10 36	+50·4 M.
9	S	17 4	10 35	5 21 N	+62·5 M.
10	**S**	17 16	11 14	0 3 S	+74·7 M.
11	M	rises	11 53	5 27	......
12	Tu	7 17	12 33	10 40	−80·8 E.
13	W	8 27	13 15	15 31	−68·6 E.
14	Th	9 38	14 0	19 48	−56·5 E.
15	F	10 53	14 47	23 19	−44·3 E.
16	S	12 4	15 37	25 50	−32·1 E.
17	**S**	13 8	16 31	27 9	−19·9 E.
18	M	14 1	17 26	27 6	− 7·7 E.
19	Tu	14 43	18 22	25 35	+ 4·5 E.
20	W	15 12	19 16	22 37	+16·7 E.
21	Th	15 37	20 9	18 19	+28·9 E.
22	F	15 56	21 1	12 52	+41·1 E.
23	S	16 9	21 52	6 35 S	+53·3 E.
24	**S**	16 25	22 44	0 12 N	+65·5 E.
25	M	16 42	23 37	7 6	+77·7 E.
26	Tu	sets	......	13 36	......
27	W	8 25	0 32	19 16	−77·8 M.
28	Th	9 58	1 30	23 37	−65·6 M.
29	F	11 23	2 31	26 21	−53·4 M.
30	S	12 33	3 32	27 19 N	−41·2 M.

MAY.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	**S**	13 26	4 33	26 35 N	−29·0 M.
2	M	14 1	5 29	24 23	−16·8 M.
3	Tu	14 28	6 21	21 0	− 4·6 M.
4	W	14 46	7 9	16 46	+ 7·6 M.
5	Th	15 1	7 53	11 57	+19·8 M.
6	F	15 12	8 34	6 45	+32·0 M.
7	S	15 25	9 14	1 22 N	+44·2 M.
8	**S**	15 35	9 53	4 3 S	+56·4 M.
9	M	15 47	10 33	9 20	+68·6 M.
10	Tu	rises	11 14	14 18	+80·8 M.
11	W	7 28	11 57	18 47	......
12	Th	8 43	12 44	22 32	−74·7 E.
13	F	9 54	13 34	25 21	−62·5 E.
14	S	11 2	14 27	26 59	−50·3 E.
15	**S**	11 59	15 22	27 15	−38·1 E.
16	M	12 44	16 17	26 6	−25·9 E.
17	Tu	13 17	17 11	23 31	−13·7 E.
18	W	13 42	18 3	19 38	− 1·5 E.
19	Th	14 0	18 54	14 38	+10·7 E.
20	F	14 17	19 43	8 47	+22·9 E.
21	S	14 30	20 32	2 20 S	+35·2 E.
22	**S**	14 46	21 22	4 21 N	+47·4 E.
23	M	15 0	22 15	10 55	+59·6 E.
24	Tu	15 21	23 11	16 54	+71·8 E.
25	W	sets	......	21 52	......
26	Th	8 53	0 11	25 22	−83·7 M.
27	F	10 13	1 13	27 7	−71·5 M.
28	S	11 14	2 16	27 3	−59·3 M.
29	**S**	12 0	3 16	25 19	−47·1 M.
30	M	12 28	4 12	22 14	−34·8 M.
31	Tu	12 51	5 2	18 10 N	−22·6 M.

JUNE.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	W	13 6	5 49	13 25 N	−10·4 M.
2	Th	13 20	6 31	8 15	+ 1·8 M.
3	F	13 34	7 12	2 52 N	+14·0 M.
4	S	13 43	7 51	2 34 S	+26·2 M.
5	**S**	13 54	8 31	7 54	+38·4 M.
6	M	14 7	9 11	12 58	+50·6 M.
7	Tu	14 22	9 54	17 36	+62·9 M.
8	W	14 42	10 40	21 35	+75·1 M.
9	Th	rises	11 29	24 41	+87·3 M.
10	F	8 53	12 22	26 38	......
11	S	9 53	13 16	27 16	−68·2 E.
12	**S**	10 42	14 12	26 26	−56·0 E.
13	M	11 19	15 7	24 8	−43·7 E.
14	Tu	11 48	16 0	20 31	−31·5 E.
15	W	12 7	16 51	15 46	−19·3 E.
16	Th	12 21	17 39	10 10	− 7·1 E.
17	F	12 36	18 27	3 59 S	+ 5·2 E.
18	S	12 52	19 15	2 29 N	+17·4 E.
19	**S**	13 7	20 5	8 55	+29·6 E.
20	M	13 24	20 58	14 57	+41·8 E.
21	Tu	13 45	21 55	20 10	+54·0 E.
22	W	14 17	22 55	24 10	+66·3 E.
23	Th	15 1	23 57	26 36	+78·5 E.
24	F	sets	......	27 14	......
25	S	9 50	0 59	26 7	−77·1 M.
26	**S**	10 28	1 58	23 29	−64·8 M.
27	M	10 54	2 52	19 40	−52·6 M.
28	Tu	11 12	3 41	15 2	−40·4 M.
29	W	11 28	4 26	9 53	−28·2 M.
30	Th	11 39	5 8	4 29 N	−16·0 M.

JULY.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	F	11 50	5 48	1 0 S	− 3·8 M.
2	S	12 1	6 27	6 24	+ 8·4 M.
3	**S**	12 13	7 7	11 33	+20·7 M.
4	M	12 27	7 49	16 19	+32·9 M.
5	Tu	12 46	8 34	20 30	+45·1 M.
6	W	13 11	9 22	23 52	+57·4 M.
7	Th	13 44	10 13	26 12	+69·6 M.
8	F	14 30	11 8	27 13	+81·8 M.
9	S	rises	12 4	26 47	......
10	**S**	9 20	13 0	24 50	−73·7 E.
11	M	9 51	13 55	21 27	−61·5 E.
12	Tu	10 12	14 47	16 52	−49·3 E.
13	W	10 30	15 37	11 21	−37·1 E.
14	Th	10 45	16 25	5 13 S	−24·8 E.
15	F	10 58	17 13	1 12 N	−12·6 E.
16	S	11 14	18 2	7 36	− 0·3 E.
17	**S**	11 29	18 52	13 39	+11·9 E.
18	M	11 50	19 46	18 59	+24·1 E.
19	Tu	12 15	20 44	23 14	+36·3 E.
20	W	12 54	21 44	26 4	+48·5 E.
21	Th	13 46	22 45	27 15	+60·7 E.
22	F	14 51	23 44	26 42	+73·0 E.
23	S	sets	......	24 34	......
24	**S**	8 54	0 40	21 6	−82·6 M.
25	M	9 14	1 31	16 40	−70·4 M.
26	Tu	9 31	2 18	11 37	−58·2 M.
27	W	9 45	3 2	6 12	−46·0 M.
28	Th	9 58	3 43	0 39 N	−33·7 M.
29	F	10 8	4 23	4 50 S	−21·5 M.
30	S	10 19	5 3	10 7	− 9·3 M.
31	**S**	10 32	5 44	15 1 S	+ 2·9 M.

AUGUST.					
Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	M	10 49	6 27	19 23 S	+15·2 M.
2	Tu	11 8	7 13	23 0	+27·4 M.
3	W	11 38	8 3	25 40	+39·6 M.
4	Th	12 20	8 56	27 8	+51·8 M.
5	F	13 16	9 52	27 11	+64·0 M.
6	S	14 26	10 49	25 43	+76·3 M.
7	**S**	rises	11 45	22 43	......
8	M	8 16	12 39	18 23	−79·2 E.
9	Tu	8 35	13 31	12 57	−67·0 E.
10	W	8 52	14 21	6 47	−54·8 E.
11	Th	9 6	15 10	0 13 S	−42·6 E.
12	F	9 20	15 59	6 21 N	−30·4 E.
13	S	9 34	16 49	12 34	−18·2 E.
14	**S**	9 53	17 42	18 6	− 6·0 E.
15	M	10 18	18 38	22 36	+ 6·3 E.
16	Tu	10 51	19 36	25 44	+18·5 E.
17	W	11 37	20 36	27 17	+30·7 E.
18	Th	12 37	21 35	27 9	+42·9 E.
19	F	13 49	22 31	25 26	+55·2 E.
20	S	15 9	23 24	22 21	+67·5 E.
21	**S**	sets	......	18 11	......
22	M	7 37	0 12	13 16	−88·0 M.
23	Tu	7 50	0 56	7 54	−75·8 M.
24	W	8 2	1 38	2 20 N	−63 6 M.
25	Th	8 15	2 19	3 15 S	−51·4 M.
26	F	8 25	2 59	8 39	−29·2 M.
27	S	8 36	3 39	13 42	−17·0 M.
28	**S**	8 53	4 21	18 16	− 4·8 M.
29	M	9 10	5 6	22 8	+ 7·4 M.
30	Tu	9 34	5 53	25 6	+19·6 M.
31	W	10 10	6 44	26 59 S	+31·8 M.

SEPTEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	10 58	7 38	27 32 S	+44·0 M.
2	F	12 1	8 34	26 38	+56·2 M.
3	S	13 19	9 30	24 12	+68·4 M.
4	**S**	14 46	10 26	20 19	+70·6 M.
5	M	16 13	11 19	15 11	+82·8 M.
6	Tu	rises	12 11	9 6	......
7	W	7 10	13 1	2 27 S	−72·8 E.
8	Th	7 25	13 51	4 24 N	−60·6 E.
9	F	7 41	14 43	11 0	−48·4 E.
10	S	7 57	15 36	16 57	−36·2 E.
11	**S**	8 20	16 32	21 51	−24·0 E.
12	M	8 50	17 31	25 22	−11·8 E.
13	Tu	9 34	18 31	27 17	+ 0·4 E.
14	W	10 29	19 30	27 31	+12·6 E.
15	Th	11 37	20 27	26 8	+24·8 E.
16	F	12 56	21 19	23 21	+37·0 E.
17	S	14 12	22 8	19 27	+49·2 E.
18	**S**	15 30	22 53	14 44	+61·4 E.
19	M	16 45	23 36	9 29	+73·6 E.
20	Tu	sets	......	3 57 N	......
21	W	6 20	0 16	1 41 S	−82·0 M.
22	Th	6 32	0 56	7 11	−69·8 M.
23	F	6 43	1 36	12 23	−57·6 M.
24	S	6 59	2 18	17 8	−45·4 M.
25	**S**	7 14	3 1	21 14	−33·2 M.
26	M	7 35	3 47	24 29	−21·0 M.
27	Tu	8 6	4 36	26 42	− 8 8 M.
28	W	8 49	5 28	27 42	+ 3·4 M.
29	Th	9 43	6 22	27 19	+15·6 M.
30	F	10 52	7 17	25 29 S	+27·8 M.

OCTOBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	S	12 13	8 11	22 13 S	+40·0 M.
2	**S**	13 37	9 4	17 38	+52·2 M.
3	M	15 6	9 56	11 56	+64·4 M.
4	Tu	16 36	10 47	5 27 S	+76·6 M.
5	W	rises.	11 38	1 29 N	......
6	Th	5 45	12 30	8 25	−79·1 E.
7	F	6 0	13 24	14 54	−66·9 E.
8	S	6 20	14 21	20 25	−54·7 E.
9	**S**	6 49	15 21	24 35	−42·5 E.
10	M	7 26	16 23	27 5	−30·3 E.
11	Tu	8 21	17 24	27 46	−18·1 E.
12	W	9 27	18 22	26 45	− 5·9 E.
13	Th	10 43	19 17	24 14	+ 6·3 E.
14	F	12 2	20 7	20 32	+18·5 E.
15	S	13 20	20 52	15 59	+30·7 E.
16	**S**	14 33	21 35	10 52	+42·9 E.
17	M	15 44	22 15	5 24 N	+55·0 E.
18	Tu	16 54	22 55	0 12 S	+67·2 E.
19	W	18 4	23 35	5 44	+79·3 E.
20	Th	sets	......	11 3	......
21	F	5 3	0 16	15 57	−76·4 M.
22	S	5 20	0 58	20 16	−64·2 M.
23	**S**	5 40	1 43	23 46	−52·0 M.
24	M	6 4	2 31	26 17	−39·9 M.
25	Tu	6 42	3 22	27 38	−27·7 M.
26	W	7 33	4 15	27 39	−15·5 M.
27	Th	8 36	5 8	26 18	− 3·4 M.
28	F	9 50	6 1	23 34	+ 8·8 M.
29	S	11 12	6 53	19 34	+21·0 M.
30	**S**	12 35	7 44	14 26	+33·1 M.
31	M	14 1	8 33	8 24 S	+45·3 M.

NOVEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Tu	15 29	9 23	1 45 S	+57·5 M.
2	W	16 58	10 13	5 11 N	+69·7 M.
3	Th	rises	11 6	11 57	+81·9 M.
4	F	4 43	12 2	18 4	......
5	S	5 18	13 2	23 0	−73·7 E.
6	**S**	6 6	14 6	26 19	−61·5 E.
7	M	7 10	15 10	27 45	−49·3 E.
8	Tu	8 26	16 12	27 17	−37·2 E.
9	W	9 46	17 10	25 7	−25·0 E.
10	Th	11 6	18 3	21 39	−12·8 E.
11	F	12 22	18 50	17 13	− 0·7 E.
12	S	13 35	19 34	12 10	+11·5 E.
13	**S**	14 44	20 15	6 45	+23·7 E.
14	M	15 55	20 55	1 11 N	+35·9 E.
15	Tu	17 5	21 34	4 22 S	+48·0 E.
16	W	18 16	22 15	9 44	+60·2 E.
17	Th	19 27	22 57	14 45	+72·4 E.
18	F	20 41	23 41	19 13	+84·5 E.
19	S	sets	......	22 57	......
20	**S**	4 9	0 28	25 45	−71·2 M.
21	M	4 43	1 18	27 23	−59·0 M.
22	Tu	5 28	2 10	27 44	−46·9 M.
23	W	6 26	3 3	26 43	−34·7 M.
24	Th	7 38	3 56	24 21	−22·5 M.
25	F	8 54	4 47	20 44	−10·4 M.
26	S	10 16	5 37	16 3	+ 1·8 M.
27	**S**	11 39	6 25	10 29	+13·9 M.
28	M	13 1	7 12	4 15 S	+26·1 M.
29	Tu	14 45	8 0	2 21 N	+38·3 M.
30	W	15 56	8 50	9 2 N	+50·5 M.

DECEMBER.

Day.		Moon.			
Month.	Week.	Sets.	Souths.	Dec.	Long. of Terminator.
		h m	h m	° ′	°
1	Th	17 28	9 43	15 21 N	+62·7 M.
2	F	19 1	10 40	20 49	+74·9 M.
3	S	rises	11 42	24 57	......
4	**S**	3 51	12 47	27 17	−80·8 E.
5	M	4 47	13 53	27 38	−68·6 E.
6	Tu	6 2	14 55	26 5	−56·4 E.
7	W	7 25	15 52	22 57	−44·2 E.
8	Th	8 48	16 44	18 41	−32·1 E.
9	F	10 7	17 30	13 40	−19·9 E.
10	S	11 22	18 13	8 14	− 7·8 E.
11	**S**	12 32	18 53	2 37 N	+ 4·4 E.
12	M	13 43	19 33	2 59 S	+16·5 E.
13	Tu	14 53	20 13	8 24	+28·7 E.
14	W	16 3	20 54	13 31	+40·9 E.
15	Th	17 15	21 38	18 7	+53·1 E.
16	F	18 29	22 24	22 3	+65·2 E.
17	S	19 38	23 13	25 6	+77·4 E.
18	**S**	sets	......	27 2	......
19	M	3 25	0 5	27 42	−78·4 M.
20	Tu	4 21	0 59	26 59	−66·3 M.
21	W	5 29	1 52	24 53	−54·1 M.
22	Th	6 45	2 45	21 31	−42·0 M.
23	F	8 5	3 35	17 3	−29·8 M.
24	S	9 24	4 22	11 44	−17·6 M.
25	**S**	10 45	5 9	5 46 S	− 5·4 M.
26	M	12 7	5 55	0 35 N	+ 6·7 M.
27	Tu	13 32	6 42	7 2	+18·9 M.
28	W	14 58	7 31	13 16	+31·1 M.
29	Th	16 26	8 25	18 53	+43·3 M.
30	F	17 57	9 22	23 27	+55·4 M.
31	S	19 26	10 25	26 30 N	+67·6 M.

List of the principal Meteor-showers of the Year, derived from recent Observations. By W. F. Denning.

Date.	Radiant-point.		Notes.	Date.	Radiant-point.		Notes.
	α °	δ °			α °	δ °	
Jan. 2 ...	**230**	**+53**	Swift; long paths.	July 28 ...	**339**	**−12**	Slow; long.
5 ...	140	+57	Swift; short.	30 ...	6	+35	Swift; streaks.
9 ...	221	+42	Swift; streaks.	31 ...	70	+50	Not swift; streaks.
17 ...	295	+53	Slow; bright.	Aug. 4 ...	30	+36	Swift; streaks.
22 ...	208	− 8	Very swift; streaks.	10 ...	**45**	**+57**	Swift; streaks.
25 ...	180	+24	Swift.	16 ...	61	+48	Swift; streaks.
Feb. 1 ...	211	+69	Slow; short.	21 ...	73	+41	Swift; streaks.
15 ...	236	+11	Swift; streaks.	22 ...	291	+60	Slow; bright.
15 ...	261	+ 4	Swift; streaks.	25 ...	334	+58	Rather swift.
20 ...	181	+34	Swift; bright.	25 ...	5	+11	Slow; short.
20 ...	263	+36	Swift; streaks.	Sept. 3 ...	354	+38	Very swift.
Mar. 1 ...	47	+45	Very slow.	4 ...	346	± 0	Slow; bright.
14 ...	175	+10	Slow; bright.	7 ...	62	+37	Swift; streaks.
14 ...	280	−14	Swift; streaks.	15 ...	48	+44	Very swift.
18 ...	316	+76	Slow; bright.	19 ...	5	+10	Slow; short.
24 ...	161	+58	Swift.	21 ...	31	+19	Slow.
27 ...	229	+32	Swift; small.	22 ...	63	+22	Swift; streaks.
28 ...	263	+62	Rather swift.	30 ...	25	+71	Small; short.
Apr. 17 ...	165	− 6	Very slow.	Oct. 2 ...	73	+41	Swift; streaks.
18 ...	213	+53	Swift; short.	2 ...	225	+52	Slow; bright.
18 ...	231	+17	Swift; short.	4 ...	133	+79	Swift; streaks.
19 ...	229	− 2	Slow; long.	4 ...	310	+77	Slowish.
20 ...	**270**	**+33**	Swift.	8 ...	77	+31	Swift; streaks.
25 ...	272	+21	Swift; short.	11 ...	13	+ 6	Slow; bright.
May 1 ...	239	+46	Small; short.	14 ...	40	+20	Rather swift.
5 ...	254	−21	Slowish.	14 ...	135	+68	Swift; streaks.
6 ...	**338**	**− 2**	Swift; streaks.	18 ...	**92**	**+15**	Swift; streaks.
7 ...	244	+ 7	Slow; bright.	20 ...	106	+12	Swift; streaks.
11 ...	231	+27	Slow; small.	29 ...	109	+23	Very swift.
15 ...	295	± 0	Swift; streaks.	Nov. 1 ...	43	+22	Slow; bright.
29 ...	264	+64	Slowish.	2 ...	55	+ 9	Slow; bright.
30 ...	333	+27	Swift; streaks.	7 ...	77	+32	Not very swift.
June 7 ...	247	−25	Slow; very bright.	13 ...	**150**	**+22**	Swift; streaks.
13 ...	310	+61	Swift; streaks.	16 ...	154	+41	Swift; streaks.
15 ...	291	+52	Swift; small.	17 ...	53	+71	Slowish.
15 ...	285	+23	Slowish.	20 ...	62	+23	Slow; bright.
18 ...	302	+24	Swift.	27 ...	**25**	**+44**	Very slow; trains.
20 ...	335	+57	Swift.	30 ...	190	+58	Swift; streaks.
28 ...	294	+39	Slow.	Dec. 4 ...	162	+58	Swift; streaks.
July 5 ...	21	+23	Swift; streaks.	4 ...	110	+25	Slowish.
11 ...	349	+53	Swift.	5 ...	105	+11	Slow.
13 ...	271	+21	Slow; faint.	6 ...	80	+23	Slow; bright.
19 ...	314	+48	Swift; short.	8 ...	145	+47	Swift; streaks.
20 ...	269	+49	Swift.	8 ...	208	+71	Rather swift.
22 ...	16	+31	Swift; streaks.	10 ...	**108**	**+33**	Swift; short.
23 ...	335	+49	Swift; short.	18 ...	134	+ 8	Very swift.
25 ...	48	+43	Swift; streaks.	22 ...	194	+67	Swift; streaks.

The radiant-points of the most brilliant showers are indicated by heavier type.

Some of the above systems remain visible for a considerable period; thus the Perseids, with maximum on August 10, form a rich display from July 25 to August 15, and the radiant-point advances from about 27° +54° to 56° +57°. On August 1 it is placed at 35° +56°, and on August 5 at 38° +57°.

Mercury (☿).

Date.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m
Jan. 1	18 9 55	20 19 S.	9·6	19 5	23 19	3 36
11	17 55 6	20 50	8·0	18 21	22 31	2 45
21	18 29 22	22 15	6·4	18 30	22 29	2 29
31	19 23 34	22 30	5·6	18 47	22 45	2 44
Feb. 10	20 25 36	20 48	5·2	18 58	23 8	3 19
20	21 31 5	16 54	5·0	18 58	23 34	4 5
Mar. 1	22 38 43	10 43	4·8	18 51	* *	5 6
11	23 48 25	2 25 S.	5·0	18 41	0 30	6 18
21	0 56 49	6 51 N.	5·8	18 21	0 59	7 37
31	1 48 24	13 56	7·6	17 54	1 11	8 25
Apr. 10	2 4 20	15 55	10·0	17 15	0 47	8 15
20	1 47 36	12 32	11·8	16 36	23 45	6 59
30	1 32 6	8 8	11·2	16 7	22 52	5 41
May 10	1 41 30	7 15	9·4	15 42	22 25	5 5
20	2 14 13	9 58	7·6	15 25	22 20	5 15
30	3 5 47	14 55	6·4	15 12	22 34	5 51
June 9	4 16 52	20 31	5·4	15 10	23 7	6 56
19	5 47 15	24 22	5·0	15 37	* *	8 23
29	7 20 58	24 1	5·2	16 28	0 49	9 10
July 9	8 39 30	20 4	5·8	17 41	1 28	9 21
19	9 39 21	14 29	6·6	18 33	1 48	9 9
29	10 21 33	8 50	7·6	19 4	1 52	8 40
Aug. 8	10 43 21	4 35	9·2	19 4	1 33	8 0
18	10 37 9	3 44	10·6	18 25	0 48	7 11
28	10 7 51	7 37	10·4	16 51	23 33	6 19
Sept. 7	10 1 37	11 24	8·0	15 50	22 52	5 55
17	10 44 28	9 35	6·0	16 6	22 59	5 51
27	11 49 16	3 9 N.	5·2	17 5	23 25	5 46
Oct. 7	12 53 50	4 31 S.	4·8	18 9	23 50	5 31
17	13 55 11	11 43	4·6	19 6	0 9	5 12
27	14 55 21	17 48	4·8	20 0	0 30	4 59
Nov. 6	15 55 45	22 25	5·2	20 51	0 51	4 50
16	16 54 53	25 9	5·8	21 34	1 11	4 51
26	17 42 58	25 36	7·2	21 41	1 19	4 56
Dec. 6	17 48 4	23 31	9·0	20 52	0 45	4 37
16	16 57 27	19 56	9·4	18 53	23 8	3 29
26	16 50 39	19 53 S.	7·4	18 12	22 28	2 46

Venus (♀).

Date.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m
Jan. 1	20 36 42	20 19 S.	11·6	21 41	1 54	6 7
11	21 26 58	16 49	12·0	21 28	2 5	6 40
21	22 14 58	12 33	12·4	21 13	2 14	7 13
31	23 1 0	7 44	12·8	20 54	2 20	7 44
Feb. 10	23 45 34	2 36 S.	13·4	20 32	2 25	8 17
20	0 29 15	2 39 N.	14·0	20 10	2 30	8 47
Mar. 1	1 12 42	7 49	14·8	19 48	2 34	9 17
11	1 56 27	12 41	15·8	19 26	2 38	9 47
21	2 40 54	17 6	16·8	19 7	2 43	10 18
31	3 26 11	20 51	18·0	18 49	2 49	10 48
Apr. 10	4 12 5	23 46	19·6	18 35	2 55	11 14
20	4 57 51	25 46	21·6	18 27	3 2	11 36
30	5 42 19	26 46	24·0	18 25	3 7	11 49
May 10	6 23 49	26 49	27·0	18 27	3 9	11 51
20	7 0 17	26 2	30·8	18 30	3 6	11 42
30	7 29 21	24 36	35·8	18 30	2 55	11 20
June 9	7 47 56	22 49	42·0	18 22	2 34	10 45
19	7 52 26	20 56	49·2	17 57	1 59	9 58
29	7 40 32	19 11	56·0	17 15	1 8	8 56
July 9	7 15 57	17 47	59·4	16 19	0 4	7 43
19	6 51 40	16 54	56·2	15 22	22 55	6 29
29	6 40 20	16 41	49·6	14 34	22 6	5 42
Aug. 8	6 45 3	16 59	42·6	13 59	21 33	5 3
18	7 3 10	17 23	36·4	13 36	21 12	4 50
28	7 30 51	17 31	31·6	13 24	21 1	4 39
Sept. 7	8 5 0	17 6	27·6	13 21	20 57	4 32
17	8 43 22	15 58	24·6	13 28	20 56	4 25
27	9 24 15	14 3	22·2	13 40	20 58	4 16
Oct. 7	10 6 28	11 23	20·2	13 58	21 0	4 4
17	10 49 22	8 3	18·6	14 20	21 4	3 49
27	11 32 37	4 12 N.	17·2	14 43	21 8	3 34
Nov. 6	12 16 14	0 2 S.	16·2	15 8	21 12	3 17
16	13 0 30	4 2	15·2	15 36	21 17	2 59
26	13 45 46	8 48	14·4	16 4	21 23	2 43
Dec. 6	14 32 28	12 56	13·6	16 34	21 30	2 28
16	15 20 58	16 37	13·0	17 3	21 39	2 16
26	16 11 26	19 37 S.	12·6	17 33	21 51	2 10

Mars (♂).

Date.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m
Jan. 1	14 52 27	15 39 S.	5·0	15 26	20 9	0 51
11	15 17 39	17 27	5·4	15 24	19 55	0 26
21	15 43 13	19 3	5·6	15 19	19 41	0 3
31	16 9 9	20 26	6·0	15 15	19 27	23 39
Feb. 10	16 35 20	21 34	6·2	15 9	19 14	23 19
20	17 1 40	22 27	6·6	15 2	19 1	23 0
Mar. 1	17 28 3	23 5	7·2	14 53	18 48	22 43
11	17 54 17	23 28	7·6	14 42	18 35	22 28
21	18 20 13	23 36	8·2	14 29	18 21	22 13
31	18 45 38	23 30	8·8	14 15	18 7	21 59
Apr. 10	19 10 18	23 13	9·6	13 57	17 52	21 47
20	19 34 2	22 46	10·4	13 40	17 36	21 34
30	19 56 33	22 12	11·4	13 19	17 19	21 21
May 10	20 17 32	21 35	12·6	12 56	17 1	21 6
20	20 36 42	20 59	14·2	12 32	16 41	20 50
30	20 53 33	20 28	15·6	12 6	16 18	20 30
June 9	21 7 32	20 9	17·4	11 38	15 52	20 6
19	21 18 2	20 5	19·2	11 9	15 23	19 37
29	21 24 14	20 23	21·4	10 36	14 50	19 3
July 9	21 25 31	21 2	23·4	10 2	14 11	18 21
19	21 21 40	22 0	25·2	9 26	13 28	17 30
29	21 13 18	23 4	26·4	8 44	12 40	16 36
Aug. 8	21 2 35	23 58	26·8	8 1	11 50	15 39
18	20 52 30	24 27	26·0	7 16	11 1	14 46
28	20 45 53	24 25	24·7	6 30	10 15	14 0
Sept. 7	20 44 29	23 53	22·8	5 45	9 35	13 25
17	20 48 33	22 56	20·8	5 3	9 0	12 57
27	20 57 33	21 38	18·8	4 25	8 29	12 34
Oct. 7	21 10 36	20 3	17·0	3 48	8 3	12 18
17	21 26 41	18 13	15·4	3 13	7 40	12 7
27	21 45 3	16 10	14·0	2 40	7 19	11 58
Nov. 6	22 5 4	13 54	12·8	2 8	7 0	11 52
16	22 29 12	11 29	11·6	1 37	6 42	11 47
26	22 48 9	8 54	10·8	1 5	6 24	11 43
Dec. 6	23 10 42	6 13	10·0	0 35	6 7	11 39
16	23 33 39	3 27	9·2	0 5	5 51	11 37
26	23 56 59	0 39 S.	8·6	23 33	5 35	11 36

Mercury.

d h d h d h d h
Gr. elong. E. ... Mar. 31 0; July 28 20; Nov. 22 21
„ W. ... Jan. 19 8; May 16 19; Sept. 11 1; Dec. 31 20
Sup. conj. ...... Mar. 5 18; June 20 5; Oct. 7 17
Inf. conj. .... Apr. 19 4; Aug. 25 15; Dec. 11 17
Stationary ..... Jan. 8 2; Apr. 8 23; May 1 12; Aug. 11 2
„ ...... Sept. 3 10; Dec. 2 0; Dec. 21 19

Venus.

d h
Gr. elong. E. .... Apr. 29 17
„ W. .... Sept. 18 17
Inf. conj. .... July 9 7
Stationary .... June 16 20; July 30 22 (d h)

Mars.

d h d h
Stationary......... July 6 12; Sept. 4 12
Opposition......... Aug. 3 18

	Jupiter (♃).						**Saturn (♄).**					
1892.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.	R.A.	Dec.	Diam.	Rises.	Transits.	Sets.
	h m s	° ′	″	h m	h m	h m	h m s	° ′	″	h m	h m	h m
Jan. 1	23 1 13	7 33 S.	35·2	22 53	4 18	9 43	12 3 55	2 1 N.	17·0	11 5	17 19	23 33
21	23 15 2	6 4	33·6	21 41	3 13	8 46	12 3 49	2 8	17·6	9 45	16 0	22 15
Feb. 10	23 30 54	4 20	32·6	20 28	2 10	7 51	12 1 9	2 31	18·0	8 21	14 38	20 55
Mar. 1	23 48 1	2 29	31·8	19 19	1 9	7 1	11 56 30	3 5	18·4	6 56	13 15	19 34
21	0 5 44	0 33 S.	31·6	18 6	0 8	6 9	11 50 51	3 43	18·4	5 29	11 51	18 13
Apr. 10	0 23 26	1 21 N.	32·0	16 53	23 4	5 17	11 45 24	4 18	18·3	4 1	10 27	16 53
30	0 40 34	3 9	32·4	15 42	22 2	4 24	11 41 19	4 42	17·8	2 37	9 4	15 31
May 20	0 56 32	4 48	33·4	14 31	20 59	3 27	11 39 22	4 51	17·2	1 16	7 44	14 12
June 9	1 10 42	6 12	35·0	13 20	19 55	2 30	11 39 52	4 43	16·8	23 58	6 25	12 54
29	1 22 18	7 18	37·0	12 7	18 47	1 27	11 42 49	4 19	16·1	22 43	5 9	11 37
July 19	1 30 28	8 1	39·4	10 53	17 37	0 22	11 47 54	3 43	15·6	21 33	3 56	10 19
Aug. 8	1 34 21	8 18	42·0	9 36	16 22	23 8	11 54 42	2 56	15·3	20 26	2 44	9 2
28	1 33 21	8 7	44·4	8 17	15 2	21 17	12 2 46	2 1	15·0	19 20	1 34	7 48
Sept. 17	1 27 38	7 29	46·6	6 57	13 38	20 20	12 11 36	1 3	14·8	18 15	0 24	6 33
Oct. 7	1 18 32	6 33	47·6	5 33	12 10	18 51	12 20 40	0 5 N.	14·8	17 7	23 11	5 15
27	1 8 39	5 35	47·2	4 9	10 41	17 18	12 29 28	0 50 S.	15·0	16 1	22 1	4 1
Nov. 16	1 0 55	4 51	45·4	2 47	9 15	15 47	12 37 27	1 38	15·2	14 54	20 50	2 46
Dec. 6	0 57 26	4 36	42·8	1 26	7 53	14 20	12 44 1	2 15	15·8	13 45	19 38	1 31
26	0 59 2	4 52 N.	40·2	0 8	6 36	13 4	12 48 38	2 39 S.	16·2	12 33	18 24	0 15

	Uranus (♅).						**Neptune (♆).**					
Jan. 1	14 13 3	12 52 S.	3·6	14 30	19 27	0 25	4 20 59	19 51 N.	2·6	1 45	9 37	17 29
31	14 15 35	13 4	3·7	12 36	17 32	22 28	4 18 57	19 48	2·6	23 44	7 36	15 28
Mar. 1	14 14 59	13 0	3·7	10 37	15 33	20 29	4 18 55	19 50	2·6	21 43	5 39	13 31
31	14 11 36	12 43	3·8	8 34	13 32	18 30	4 20 58	19 56	2·6	19 46	3 43	11 36
Apr. 30	14 6 51	12 18	3·8	6 28	11 29	16 30	4 24 40	20 6	2·5	17 51	1 49	9 43
May 30	14 2 28	11 55	3·7	4 24	9 27	14 30	4 29 14	20 17	2·5	16 0	23 51	7 47
June 29	14 0 5	11 43	3·7	2 23	7 27	12 31	4 33 49	20 26	2·5	14 6	21 58	5 54
July 29	14 0 31	11 46	3·6	0 30	5 29	10 32	4 37 31	20 33	2·6	12 11	20 4	4 2
Aug. 28	14 3 51	12 6	3·6	22 28	3 34	8 37	4 39 40	20 36	2·6	10 15	18 8	2 5
Sept. 27	14 9 30	12 36	3·5	20 39	1 42	6 39	4 39 45	20 35	2·6	8 13	16 10	0 3
Oct. 27	14 16 31	13 13	3·5	18 53	23 48	4 46	4 37 52	20 30	2·7	6 14	14 10	22 2
Nov. 26	14 23 39	13 48	3·6	17 5	21 57	2 52	4 34 37	20 23	2·7	4 14	12 9	20 4
Dec. 26	14 29 36	14 18 S.	3·6	15 15	20 4	0 58	4 31 10	20 16 N.	2·7	2 13	10 8	18 3

	Conjunction.	Opposition.	Quadrature.		Stationary.	
	d h	d h	d h	d h	d h	d h
Jupiter	Mar. 20 16	Oct. 12 7	July 15 4		Aug. 14 1 ;	Dec. 9 19
Saturn	Sept. 25 10	Mar. 16 9	June 13 20		Jan. 10 5 ;	May 25 21
Uranus	Oct. 28 21	Apr. 23 7	Jan. 25 23 ;	July 24 2	Feb. 9 3 ;	July 9 15
Neptune......	May 29 6	Dec. 1 2	Feb. 24 21 ;	Sept. 2 22	Feb. 15 13 ;	Sept. 13 8

Ceres.				**Pallas.**			
1892.	R.A.	Dec.	Transits.	1892.	R.A.	Dec.	Transits.
	h m	° ′	h m		h m	° ′	h m
Oct. 3 ...	4 4·0	12 14 N.	15 11	Aug. 4 ...	0 25·9	3 46 N.	15 29
27 ...	3 53·0	11 57	13 25	28 ...	0 19·8	0 24 S.	13 49
Nov. 20 ...	3 31·4	11 46	11 29	Sept. 21 ...	0 4·2	6 8	11 59
Dec. 14 ...	3 11·3	12 12 N.	9 35	Oct. 15 ...	23 47·0	11 35	10 7
				Nov. 8 ...	23 38·2	15 1	8 24
				Dec. 2 ...	23 42·4	16 9	6 54
				26 ...	23 58·2	15 34 S.	5 36

Opposition:—*Ceres*, Nov. 15; *Pallas*, Sept. 20.

Vesta.

1892.	R.A.	Dec.	Transits.
	h m	° ′	h m
Oct. 3 ...	3 52·6	10 53 N.	14 59
27 ...	3 38·7	9 42	13 11
Nov. 20 ...	3 14·6	8 44	11 12
Dec. 14 ...	2 55·2	8 48 N.	9 19

Opposition:—*Vesta*, Nov. 12.

ECLIPSES IN 1892.

I.—*A Total Eclipse of the Sun, April* 26.

Invisible at Greenwich. Total phase visible only in the South Pacific. Partial phase visible near Sunset on the western coast of South America, and near Sunrise in New Zealand. The conditions of the Eclipse render it of practically no scientific interest.

II.—*A Partial Eclipse of the Moon, May* 11.

Visible at Greenwich. At Mid-Eclipse the Moon is in the zenith of 15½° E., 19° S. The first Contact with the Shadow occurs at 82° to E. of N., and the last at 41° to W. of N. (for *Direct* image).

		d	h	m		h	m
First Contact with Penumbra ...	May	11	7	55·9	Shadow	9	10·2
Second „ „ „	„	11	13	50·9	„	12	36·6

Magnitude of Eclipse (Moon's diameter = 1) 0·953.

III.—*A Partial Eclipse of the Sun, October* 20.

Invisible in Europe, Asia, and Africa. Visible throughout nearly the whole of North America.

Greenwich M.T. of ☌ in R.A. Oct $20^d\ 5^h\ 34^m\ 47^s.7$.

☉'s and ☾'s R.A. $13^h\ 43^m\ 15^s.21$. Hourly motions $9^s.47$ and $108^s.08$.

	°	′	″			′	″
☉'s Declination	10	41	55·7 S.	Hourly motion		0	53·6
☾'s Declination	9	39	55·5	Hourly motion		13	6·3
☉'s Equat. Hor. Parallax		0	8·9	True Semidiameter......		16	6·7
☾'s Equat. Hor. Parallax		54	1·8	True Semidiameter		14	44·8

		d	h	m		°	′		°	′
Eclipse begins......	Oct.	20	4	15·0	in Long.	133	22 W.,	Lat.	65	25 N.
Eclipse ends			8	57·4		50	52 W.,		14	2 N.

IV.—*A Total Eclipse of the Moon, November* 4.

Partial Eclipse at Greenwich, the Moon rising at $4^h\ 21^m$. At Mid-Eclipse the Moon is in the zenith of 119° 29′ E., 16° 10′ N. The first Contact with the Shadow occurs at 91° to E. of N.; the last at 138° to W. (for *Direct* image).

		d	h	m		h	m
First Contact with Penumbra ...	Nov.	4	1	11·6	Shadow	2	9·2
Last „ „ „	„	6	18	4	„	5	20·8

Total from $3^h\ 23^m$ to $4^h\ 7^m.0$.

Magnitude of Eclipse (Moon's diameter = 1) 1·092.

OCCULTATIONS, 1892. (VISIBLE AT GREENWICH.)

Date.	Star.	Mag.	Disap. M.T.	Disap. P.	Reap. M.T.	Reap. P.
			h m	°	h m	°
Jan.						
8	σ Arietis ...	6	12 42	35	13 29	286
12	40 Geminor.	6½	18 57	37	19 22	337
16	42 Leonis ...	6	11 17	163	12 5	243
16	B.A.C. 3579	6	15 43	105	16 55	318
18	*b* Virginis .	6	17 9	178	17 57	254
19	γ^1 Virginis .	2½	16 2	168	17 3	266
19	B.A.C. 4277	6	17 6	121	18 26	312
22	8 Libræ ...	6	13 39‡	125	14 42	294
22	α Libræ ...	2½	13 47‡	129	14 51	291
Feb.						
1	B.A.C. 8274	6	5 58	32	6 55	263
2	15 Ceti	6½	3 49	55	4 59	233
3	μ Piscium .	5	3 59	22	4 59	268
6	κ^1 Tauri ...	4½	10 19	136	10 55	201
6	v^1 Tauri ...	4½	11 3	12	11 30	326
7	118 Tauri...	6	12 46	101	13 45	257
10	λ Cancri ...	6	10 45	72	11 50	322
16	46 Virginis.	6	9 12‡	55	9 39	2
23	τ Sagittarii .	3½	17 17‡	130	18 13	236
Mar.						
8	ω^2 Cancri...	6½	8 14	43	8 55	342
11	*i* Leonis ...	6	6 20	102	7 27	301
14	B.A.C. 4277	6	6 26‡	111	7 24	303
16	λ Virginis..	4½	12 21	119	13 39	310
16	Uranus......	...	12 30	112	13 46	318
18	δ Scorpii ..	2½	17 16	87	18 32	311
19	25 Scorpii .	6	14 35	178	15 5	226
Apr.						
2	139 Tauri...	5½	12 36	122	13 19	241
3	39 Geminor.	6½	12 59	136	13 39	240
4	*c* Geminor. .	6	6 24	73	7 35	309
7	42 Leonis ...	6	7 50	171	8 40	247
7	B.A.C. 3579	6	12 18	83	13 12	338
9	*b* Virginis .	6	14 4	81	14 54	343
May						
4	η Leonis ...	3½	6 26	93	7 38	326
8	θ Virginis..	4½	11 50	151	12 59	278
9	B.A.C. 4591	6	7 1	134	8 12	296
12	19 Scorpii. .	6	15 22	109	16 36†	276
15	τ Sagittarii.	3½	15 59	15	16 29	333
19	τ^1 Aquarii .	6	15 4	61	16 13	247
June						
4	38 Virginis .	6	9 32	199	9 55	234
6	λ Virginis..	4½	7 40	88	8 44	341
8	δ Scorpii ...	2½	12 12	51	12 56	346
9	25 Scorpii..	6	8 39	142	9 43	261
10	3 Sagittarii.	5	12 8	160	12 48	218
20	σ Arietis ...	6	13 55	85	14 42	225
July						
3	Uranus......	...	9 10	81	10 10	338
9	B.A.C. 6666	6	15 6	6	15 30†	326
11	35 Capricor.	6	15 52	84	16 55	221
19	v^1 Tauri ...	4½	12 13*	58	12 59	267
19	v^2 Tauri ...	6	12 42	32	13 19	292
Aug.						
2	B.A.C. 5354	6½	7 49	109	9 12	290
4	B.A.C. 6063	6½	7 34	135	8 39	243
6	A Sagittarii	5	10 52	359	11 8	336
7	B.A.C. 7237	6	7 51*	95	8 58	240
9	τ^2 Aquarii .	4	10 54	106	11 43	195
11	14 Ceti	6½	11 55	104	12 40	188
19	λ Cancri ...	6	15 2	89	15 56	280
Sept.						
3	B.A.C. 7077	6	10 6	74	11 21	244
4	35 Capricor.	6	8 33	40	9 39	276
6	ψ^3 Aquarii .	5	10 9	348	10 32	309
10	53 Arietis...	6	16 26	72	17 37	236
11	A Tauri ...	4½	15 39	136	16 5	178
12	B.A.C. 1518	6	11 20	130	11 49	194
12	*k* Tauri......	5½	12 3	54	12 58	268
14	47 Geminor.	6	16 37	63	17 42	298
15	ω^2 Cancri...	6½	12 53	71	13 43	295
28	B.A.C. 6120	6½	4 54	74	6 13	295
28	B.A.C. 6127	5	5 40	90	7 4	274
Oct.						
3	τ^2 Aquarii .	4	6 31	88	7 33	216
4	B.A.C. 8274	6	9 52	107	10 34	181
5	14 Ceti	6½	6 23	123	6 50	177
6	B.A.C. 481 .	6½	9 35	71	10 37	220
6	*o* Piscium...	4	15 6	83	16 4	218
8	B.A.C. 1143	6	16 16	121	17 0	200
12	*c* Geminor. .	6	11 11	95	12 5	266
15	B.A.C. 3579	6	13 47	90	14 42	307
15	*i* Leonis ...	6	15 32	69	16 20	335
28	B.A.C. 7108	6½	5 34	122	6 24	198
Nov.						
4	53 Arietis...	6	11 42	81	12 47	222
5	A Tauri ...	4½	9 10	112	9 53	199
6	*k* Tauri ...	5½	5 15*	46	5 54	285
9	λ Cancri ...	6	9 49	175	9 58	195
9	v^2 Cancri...	6½	12 45	81	13 50	297
9	v^3 Cancri ...	6	14 14	70	15 17	315
9	32 Cancri...	6	15 11	46	15 51	344
11	42 Leonis...	6	16 58	123	18 16	296
23	B.A.C. 6628	6	3 42	80	5 3	257
28	B.A.C. 8274	6	5 9	104	5 55	185
30	B.A.C. 481 .	6½	8 8	127	8 29	162
30	*o* Piscium...	4	13 30	105	14 13	203
Dec.						
2	B.A.C. 1143	6	14 45	123	15 25	207
3	*k* Tauri......	5½	18 20	116	19 5	236
5	47 Geminor.	6	18 33	33	18 54	352
6	ω^1 Cancri...	6	11 18	76	12 23	297
6	ω^2 Cancri...	6½	11 53	139	12 45	235
25	B.A.C. 8274	6½	6 5	79	7 9	208
29	53 Arietis...	6	9 6	145	9 17	162
31	*k* Tauri ...	5½	3 0	64	3 49	264

* Star setting. † Star rising. ‡ Star below horizon.

The angles (P) are reckoned from the true N. point in the direction N., E., S., W., *i.e.* from the bottom of the Moon's inverted image towards the right.

The following "near approaches" are also given in the 'Nautical Almanac,' and are especially interesting to observers rather South and rather North of Greenwich respectively:—

North Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°	Aug.			h m	°
11	118 Tauri ...	6	5 16	344	6	ω Sagittorii..	5	9 0	352
12	39 Geminor..	6½	18 55	8	Sept.				
16	*i* Leonis......	6	18 21	30	4	37 Capricor..	6	13 43	332
Apr.					27	B.A.C. 5800.	6½	6 53	6
10	γ^1 Virginis ...	2½	13 47	34	Oct.				
May					31	ψ^3 Aquarii...	5	7 33	326
6	B.A.C. 3996.	6	14 38	28					

South Near Approaches.

Date.	Star.	Mag.	G.M.T.	Angle.	Date.	Star.	Mag.	G.M.T.	Angle.
Jan.			h m	°	Apr.			h m	°
10	τ Tauri	4½	11 40	168	3	40 Geminor..	6½	13 35	188
18	B.A.C. 3996.	6	9 40	203					
Feb.					Aug.				
6	κ^2 Tauri......	6½	10 38	168	10	B.A.C. 8274.	6	15 13	144
16	θ Virginis ...	4½	17 9	216					
17	B.A.C. 4591.	6	12 25	213	Sept.				
Mar.					11	39 Tauri......	6	16 14	158
7	39 Geminor..	6½	5 30	177					
13	*b* Virginis ...	6	6 31	205	Nov.				
14	38 Virginis..	6	14 21	217	5	39 Tauri......	6	9 45	155
31	13 Tauri......	5½	7 9	164					

CONFIGURATIONS OF JUPITER'S SATELLITES

FOR AN INVERTING TELESCOPE.

Day.	Jan. 7^h	Feb. $6^h\ 30^m$	May. $15^h\ 30^m$	June. 15^h	July. 14^h	Aug. 13^h	Sept. 12^h	Oct. 11^h	Nov. 10^h	Dec. 9^h	Day.
1	♃42O3	342O●	42O3●	♃42O3	34O12	21O4●	♃O134	2O341	4321O	41O2●	1
2	4O132	4321O	41O23	432O1	34O2●	♃O234	2O34●	31O24	432O1	4O123	2
3	431O2	43O12	♃O413	431O2	4231O	O1423	31O24	♃32O4	O423●	421O3	3
4	432O1	41O2●	231O4	♃43O1	42O13	241O3	3O124	23O14	12O43	♃42O3	4
5	3412O	42O13	3O214	241O3	41O23	423O1	321O4	1O234	2O134	43O12	5
6	34O12	412O3	31O24	O2413	♃4O13	431O2	24O1●	O1234	13O24	3142O	6
7	12O43	4O132	2O14●	1O243	421O3	43O21	41O23	21O34	3O124	32O14	7
8	2O134	4321O	2O34●	2O134	43O21	4231O	4O213	2O314	321O4	1O24●	8
9	O234●	♃32O●	1O234	32O14	34O2●	4O123	42O3●	31O42	32O14	O1234	9
10	31O24	3O124	O2134	31O24	♃32O4	4O23●	♃43O2	♃34O1	1O324	21O34	10
11	32O14	13O24	213O4	3O214	2O134	421O3	43O12	423O1	♃1O43	♃2O34	11
12	312O4	2O134	34O21	21O34	1O234	23O41	4321O	41O23	24O13	3O24●	12
13	3O124	12O34	431O2	O2143	O2134	31O24	42O1●	4O123	413O2	♃31O4	13
14	♃1O43	O1324	42O1●	1O423	21O34	3O214	14O23	421O3	43O12	32O14	14
15	24O13	♃31O4	421O3	42O13	3O14●	231O4	O2143	42O31	4321O	143O2	15
16	41O23	32O14	♃4O23	423O●	31O24	O134●	21O34	431O2	432O1	4O123	16
17	♃41O2	3O42●	4O123	431O2	♃32O4	O234●	♃3O24	34O12	41O32	421O3	17
18	432O1	431O2	4213O	43O21	24O13	21O34	3O124	32O4●	♃4O23	42O13	18
19	4312O	42O13	34O1●	4213O	41O23	2O314	321O4	1O324	24O13	43O2●	19
20	43O12	421O3	31O42	4O13●	4O213	31O42	23O14	O1234	1O324	431O2	20
21	412O3	4O123	32O14	41O23	421O3	34O21	1O234	12O34	3O124	432O1	21
22	42O13	413O2	21O34	42O13	432O1	4231O	O2413	2O314	312O4	413O●	22
23	1O3●●	432O1	O1234	231O4	431O2	4O31●	241O3	31O24	32O14	4O123	23
24	♃O324		O234●	♃3O24	432O1	41O23	43O1●	3O214	1O324	21O43	24
25	32O14		♃21O4	3O124	42O●●	♃42O3	43O2●	321O4	O1234	2O134	25
26	321O4		3O14●	213O4	14O23	42O13	4321O	♃4O●●	2O34●	31O24	26
27	3O124		31O24	O134●	O1243	431O2	423O1	4O123	1O34●	♃3O24	27
28	1O234		32O41	1O234	21O34	34O12	41O23	412O3	3O412	32O14	28
29	2O134		241O3	2O134	3O14●	321O4	4O213	42O13	3412O	31O4●	29
30	1O43●		4O123	213O4	31O24	2O314	241O3	413O2	432O1	O1324	30
31	O1432		4O23●		32O14	1O234		43O21		12O43	31

The circle (O) represents Jupiter; ♃ signifies that the satellite is on the disk; ● signifies that the satellite is behind the disk or in the shadow.

JUPITER'S SATELLITES, 1892.

Phenomena.

e, Signifies Ec. dis.; E, Ec. re.; *o*, Occ. dis.; O, Occ. re.; *t*, Tr. Ing.; T, Tr. Egr.

January.

d	h	m	
1	5	42	i. *t*.
	8	0	i. T.
	17	57	ii. *o*.
	23	9	ii. E.
2	3	2	i. *o*.
	6	29	i. E.
	19	42	iii. *t*.
	23	9	iii. T.
3	0	12	i. *t*.
	2	30	i. T.
	13	5	ii. *t*.
	15	58	ii. T.
	21	32	i. *o*.
4	0	58	i. E.
	18	42	i. *t*.
	21	0	i. T.
5	7	20	ii. *o*.
	12	29	ii. E.
	16	2	i. *o*.
	19	27	i. E.
6	9	50	iii. *o*.
	10	40	iv. *o*.
	13	12	i. *t*.
	13	15	iii. O.
	14	42	iii. *e*.
	14	44	iv. O.
	15	31	i. T.
	17	41	iii. E.
	22	8	iv. *e*.
7	0	52	iv. E.
	2	28	ii. *t*.
	5	20	ii. T.
	10	32	i. *o*.
	13	56	i. E.
8	7	42	i. *t*.
	10	1	i. T.
	20	44	ii. *o*.
9	1	48	ii. E.
	5	2	i. *o*.
	8	24	i. E.
10	0	4	iii. *t*.
	2	12	i. T.
	3	29	iii. T.
	4	31	i. T.
	15	51	ii. *t*.
	18	43	ii. T.
	23	32	i. *o*.
11	2	53	i. E.
	20	42	i. *t*.
	23	1	i. T.
12	10	7	ii. *o*.
	15	7	ii. E.

Jan. (con.).

d	h	m	
12	18	3	i. *o*.
	21	22	i. E.
13	14	11	iii. *o*.
	15	13	i. *t*.
	17	31	i. T.
	17	36	iii. O.
	18	44	iii. *e*.
	21	41	iii. E.
14	5	14	ii. *t*.
	8	7	ii. T.
	12	33	i. *o*.
	15	51	i. E.
	22	33	iv. *t*.
15	2	32	iv. T.
	9	43	i. *t*.
	12	1	i. T.
	23	32	ii. *o*.
16	4	25	ii. E.
	7	3	i. *o*.
	10	18	i. E.
17	4	13	i. *t*.
	4	27	iii. *t*.
	6	31	i. T.
	7	51	iii. T.
	18	38	ii. *t*.
	21	30	ii. T.
18	1	33	i. *o*.
	4	47	i. E.
	22	43	i. *t*.
19	1	1	i. T.
	12	57	ii. *o*.
	17	45	ii. E.
	20	3	i. *o*.
	23	17	i. E.
20	17	14	i. *t*.
	18	36	iii. *o*.
	19	32	i. T.
	21	59	iii. O.
	22	46	iii. *e*.
21	1	43	iii. E.
	8	1	ii. *t*.
	10	53	ii. T.
	14	33	i. *o*
	17	46	i. E.
22	11	44	i. *t*.
	14	2	i. T.
23	2	21	ii. *o*.
	6	55	iv. *o*.
	7	4	ii. E.
	9	4	i. *o*.
	10	50	iv. O.
	12	15	i. E.

Jan. (con.).

d	h	m	
23	16	30	iv. *e*.
	19	1	iv. E.
24	6	14	i. *t*.
	8	33	i. T.
	8	53	iii. *t*.
	12	16	iii. T.
	21	25	ii. *t*.
25	0	17	ii. T.
	3	34	i. *o*.
	6	43	i. E.
26	0	45	i. *t*.
	3	3	i. T.
	15	47	ii. *o*.
	20	23	ii. E.
	22	4	i. *o*.
27	1	12	i. E.
	19	15	i. *t*.
	21	33	i. T.
	23	2	iii. *o*.
28	2	25	iii. O.
	2	48	iii. *e*.
	5	44	iii. E.
	10	49	ii. *t*.
	13	41	ii. T.
	16	35	i. *o*.
	19	41	i. E.
29	13	46	i. *t*.
	16	4	i. T.
30	5	12	ii. *o*.
	9	42	ii. E.
	11	5	i. *o*.
	14	10	i. E.
31	8	16	i. *t*.
	10	34	i. T.
	13	20	iii. *t*.
	16	42	iii. T.
	19	2	iv. *t*.
	22	51	iv. T.

February.

d	h	m	
1	0	14	ii. *t*.
	3	5	ii. T.
	5	35	i. *o*.
	8	38	i. E.
2	2	46	i. *t*.
	5	4	i. T.
	18	38	ii. *o*.
	23	1	ii. E.
3	0	6	i. *o*.
	3	7	i. E.

Feb. (con.).

d	h	m	
3	21	17	i. *t*.
	23	35	i. T.
4	3	31	iii. *o*.
	6	51	iii. O.
	6	51	iii. *e*.
	9	45	iii. E.
	13	38	ii. *t*.
	16	30	ii. T.
	18	36	i. *o*.
	21	36	i. E.
5	15	48	i. *t*.
	18	6	i. T.
6	8	4	ii. *o*.
	12	20	ii. E.
	13	6	i. *o*.
	16	5	i. E.
7	10	18	i. *t*.
	12	36	i. T.
	17	49	iii. *t*.
	21	9	iii. T.
8	3	3	ii. *t*.
	5	54	ii. T.
	7	37	i. *o*.
	10	34	i. E.
9	3	37	iv. *o*.
	4	49	i. *t*.
	7	7	i. T.
	7	20	iv. O.
	10	52	iv. *e*.
	13	9	iv. E.
	21	30	ii. *o*.
10	1	40	ii. E.
	2	7	i. *o*.
	5	2	i. E.
	23	19	i. *t*.
11	1	37	i. T.
	8	1	iii. *o*.
	13	46	iii. E.
	16	28	ii. *t*.
	19	19	ii. T.
	20	38	i. *o*.
	23	31	i. E.
12	17	50	i. *t*.
	20	8	i. T.
13	10	56	ii. *o*.
	14	58	ii. E.
	15	8	i. *o*.
	18	0	i. E.
14	12	21	i. *t*.
	14	39	i. T.
	22	20	iii. *t*.
15	1	38	iii. T.

Feb. (con.).

d	h	m	
15	5	53	ii. *t*.
	8	43	ii. T.
	9	39	i. *o*.
	12	28	i. E.
16	6	51	i. *t*.
	9	9	i. T.
17	0	23	ii. *o*.
	4	9	i. *o*.
	4	18	ii. E.
	6	57	i. E.
	15	54	iv. *t*.
	19	30	iv. T.
18	1	22	i. *t*.
	3	40	i. T.
	12	32	iii. *o*.
	17	47	iii. E.
	19	18	ii. *t*.
	22	8	ii. T.
	22	40	i. *o*.
19	1	26	i. E.
	19	53	i. *t*.
	22	10	i. T.
20	13	49	ii. *o*.
	17	10	i. *o*.
	17	36	ii. E.
	19	54	i. E.
21	14	23	i. *t*.
	16	41	i. T.
22	2	52	iii. *t*.
	6	8	iii. T.
	8	43	ii. *t*.
	11	33	ii. T.
	11	41	i. *o*.
	14	23	i. E.
23	8	54	i. *t*.
	11	12	i. T.

May.

d	h	m	
1	12	48	i. *e*.
	15	45	i. O.
	17	17	ii. *e*.
	21	25	ii. O.
2	10	48	i. *t*.
	13	4	i. T.
3	7	17	i. *e*.
	10	15	i. O.
	13	8	ii. *t*.
	13	57	iv. *o*.
	14	35	iv. O.
	15	49	ii. T.

May (con.).

d	h	m	
4	0	22	iii. *t*.
	3	9	iii. T.
	5	19	i. *t*.
	7	34	i. T.
5	1	45	i. *e*.
	4	45	i. O.
	6	37	ii. *e*.
	10	50	ii. O.
	23	49	i. *t*.
6	2	4	i. T.
	20	14	i. *e*.
	23	15	i. O.
7	2	32	ii. *t*.
	5	13	ii. T.
	11	14	iii. *e*.
	13	51	iii. E.
	14	26	iii. *o*.
	17	12	iii. O.
	18	19	i. *t*.
	20	35	i. T.
8	14	42	i. *e*.
	17	45	i. O.
	19	55	ii. *e*.
9	0	14	ii. O.
	12	49	i. *t*.
	15	5	i. T.
10	9	11	i. *e*.
	12	15	i. O.
	15	56	ii. *t*.
	18	36	ii. T.
11	4	51	iii. *t*.
	7	20	i. *t*.
	7	34	iii. T.
	9	35	i. T.
12	3	39	i. *e*.
	6	45	i. O.
	9	14	ii. *e*.
	13	39	ii. O.
13	1	50	i. *t*.
	4	5	i. T.
	22	8	i. *e*.
14	1	15	i. O.
	5	20	ii. *t*.
	8	0	ii. T.
	15	16	iii. *e*.
	17	51	iii. E.
	18	55	iii. *o*.
	20	20	i. *t*.
	21	36	iii. O.
	22	35	i. T.
15	16	36	i. *e*.
	19	45	i. O.

May (con.).

d	h	m	
15	22	32	ii. *e*.
16	3	3	ii. O.
	14	50	i. *t*.
	17	5	i. T.
17	11	5	i. *e*.
	14	15	i. O.
	18	44	ii. *t*.
	21	23	ii. T.
18	9	18	iii. *t*.
	9	20	i. *t*.
	11	35	i. T.
	11	58	iii. E.
19	5	33	i. *e*.
	8	45	i. O.
	11	51	ii. *e*.
	16	27	ii. O.
20	3	50	i. *t*.
	6	5	i. T.
21	0	2	i. *e*.
	3	15	i. O.
	8	7	ii. *t*.
	10	46	ii. T.
	19	17	iii. *e*.
	21	51	iii. E.
	22	20	i. *t*.
	23	21	iii. *o*.
22	0	35	i. T.
	1	59	iii. O.
	18	30	i. *e*.
	21	45	i. O.
23	1	9	ii. *e*.
	5	51	ii. O.
	16	50	i. *t*.
	19	5	i. T.
24	12	59	i. *e*.
	16	15	i. O.
	21	31	ii. *t*.
25	0	8	ii. T.
	11	20	i. *t*.
	13	35	i. T.
	13	43	iii. *t*.
	16	19	iii. T.
26	7	27	i. *e*.
	10	44	i. O.
	14	28	ii. *e*.
	19	14	ii. O.
27	5	50	i. *t*.
	8	4	i. T.
28	1	56	i. *e*.
	5	14	i. O.
	10	54	ii. *t*.
	13	31	ii. T.
	23	18	iii. *e*.
29	0	20	i. *t*.
	1	51	iii. E.
	2	35	i. T.
	3	45	iii. *o*.
	6	19	iii. O.
	20	24	i. *e*.
	23	44	i. O.
30	3	46	ii. *e*.

May (con.).

d	h	m	
30	8	37	ii. O.
	18	50	i. *t*.
	21	4	i. T.
31	14	53	i. *e*.
	18	13	i. O.

June.

d	h	m	
1	0	16	ii. *t*.
	2	53	ii. T.
	13	19	i. *t*.
	15	34	i. T.
	18	7	iii. *t*.
	20	39	iii. T.
2	9	21	i. *e*.
	12	43	i. O.
	17	5	ii. *e*.
	22	0	ii. O.
3	7	49	i. *t*.
	10	4	i. T.
4	3	50	i. *e*.
	7	13	i. O.
	13	39	ii. *t*.
	16	15	ii. T.
5	2	19	i. *t*.
	3	19	iii. *e*.
	4	33	i. T.
	5	50	iii. E.
	8	6	iii. *o*.
	10	36	iii. O.
	22	18	i. *e*.
6	1	42	i. O.
	6	23	ii. *e*.
	11	23	ii. O.
	20	49	i. *t*.
	23	3	i. T.
7	16	47	i. *e*.
	20	12	i. O.
8	3	1	ii. *t*.
	5	37	ii. T.
	15	18	i. *t*.
	17	33	i. T.
	22	28	iii. *t*.
9	0	56	iii. T.
	11	15	i. *e*.
	14	41	i. O.
	19	42	ii. *e*.
10	0	45	ii. O.
	9	48	i. *t*.
	12	2	i. T.
11	5	44	i. *e*.
	9	11	i. O.
	16	23	ii. *t*.
	18	58	ii. T.
12	4	17	i. *t*.
	6	32	i. T.
	7	20	iii. *e*.
	9	50	iii. E.
	12	25	iii. *o*.
	14	51	iii. O.

June (con.).

d	h	m	
13	0	12	i. *e*.
	3	40	i. O.
	9	0	ii. *e*.
	14	6	ii. O.
	22	47	i. *t*.
14	1	1	i. T.
	18	41	i. *e*.
	22	9	i. O.
15	5	45	ii. *t*.
	8	19	ii. T.
	17	16	i. *t*.
	19	30	i. T.
16	2	46	iii. *t*.
	5	10	iii. T.
	13	9	i. *e*.
	16	39	i. O.
	22	18	ii. *e*.
17	0	52	ii. E.
	0	54	ii. *o*.
	3	27	ii. O.
	11	46	i. *t*.
	14	0	i. T.
18	7	38	i. *e*.
	11	8	i. O.
	19	6	ii. *t*.
	21	39	ii. T.
19	6	15	i. *t*.
	8	29	i. T.
	11	21	iii. *e*.
	13	50	iii. E.
	16	42	iii. *o*.
	19	4	iii. O.
20	2	6	i. *e*.
	5	37	i. O.
	11	36	ii. *e*.
	14	9	ii. E.
	14	15	ii. *o*.
	16	48	ii. O.
21	0	45	i. *t*.
	2	58	i. T.
	20	35	i. *e*.
22	0	7	i. O.
	8	27	ii. *t*.
	11	0	ii. T.
	19	14	i. *t*.
	21	27	i. T.
23	7	1	iii. *t*.
	9	21	iii. T.
	15	3	i. *e*.
	18	36	i. O.
24	0	55	ii. *e*.
	3	27	ii. E.
	3	36	ii. *o*.
	6	8	ii. O.
	13	43	i. *t*.
	15	57	i. T.
25	9	32	i. *e*.
	13	5	i. O.
	21	47	ii. *t*.
26	0	20	ii. T.
	8	12	i. *t*.

June (con.).

d	h	m	
26	10	26	i. T.
	15	22	iii. *e*.
	17	50	iii. E.
	20	56	iii. *o*.
	23	13	iii. O.
27	4	0	i. *e*.
	7	34	i. O.
	14	12	ii. *e*.
	16	45	ii. E.
	16	56	ii. *o*.
	19	28	ii. O.
28	2	41	i. *t*.
	4	55	i. T.
	22	29	i. *e*.
29	2	3	i. O.
	11	7	ii. *t*.
	13	39	ii. T.
	21	10	i. *t*.
	23	24	i. T.
30	11	13	iii. *t*.
	13	28	iii. T.
	16	57	i. *e*.
	20	32	i. O.

July.

d	h	m	
1	3	31	ii. *e*.
	6	3	ii. E.
	6	16	ii. *o*.
	8	48	ii. O.
	15	39	i. *t*.
	17	53	i. T.
2	11	26	i. *e*.
	15	1	i. O.
3	0	27	ii. *t*.
	2	58	ii. T.
	10	8	i. *t*.
	12	21	i. T.
	19	24	iii. *e*.
	21	50	iii. E.
4	1	6	iii. *o*.
	3	19	iii. O.
	5	54	i. *e*.
	9	29	i. O.
	16	48	ii. *e*.
	19	20	ii. E.
	19	36	ii. *o*.
	22	7	ii. O.
5	4	37	i. *t*.
	6	50	i. T.
6	0	23	i. *e*.
	3	58	i. O.
	13	46	ii. *t*.
	16	17	ii. T.
	23	6	i. *t*.
7	1	19	i. T.
	15	21	iii. *t*.
	17	32	iii. T.
	18	51	i. *e*.
	22	27	i. O.

July (con.).

d	h	m	
8	6	7	ii. *e*.
	8	38	ii. E.
	8	55	ii. *o*.
	11	25	ii. O.
	17	35	i. *t*.
	19	48	i. T.
9	13	20	i. *e*.
	16	56	i. O.
10	3	5	ii. *t*.
	5	36	ii. T.
	12	3	i. *t*.
	14	16	i. T.
	23	25	iii. *e*.
11	1	50	iii. E.
	5	13	iii. *o*.
	7	22	iii. O.
	7	48	i. *e*.
	11	24	i. O.
	19	24	ii. *e*.
	21	56	ii. E.
	22	14	ii. *o*.
12	0	44	ii. O.
	6	32	i. *t*.
	8	45	i. T.
13	2	17	i. *e*.
	5	53	i. O.
	16	24	ii. *t*.
	18	54	ii. T.
14	1	0	i. *t*.
	3	14	i. T.
	19	26	iii. *t*.
	20	45	i. *e*.
	21	33	iii. T.
15	0	21	i. O.
	8	42	ii. *e*.
	11	13	ii. E.
	11	32	ii. *o*.
	14	1	ii. O.
	19	29	i. *t*.
	21	42	i. T.
16	15	14	i. *e*.
	18	50	i. O.
17	5	41	ii. *t*.
	8	11	ii. T.
	13	57	i. *t*.
	16	10	i. T.
18	3	26	iii. *e*.
	5	49	iii. E.
	9	15	iii. *o*.
	9	42	i. *e*.
	11	20	iii. O.
	13	18	i. O.
	22	0	ii. *e*.
19	0	31	ii. E.
	0	49	ii. *o*.
	3	18	ii. O.
	8	26	i. *t*.
	10	39	i. T.
20	4	11	i. *e*.
	7	47	i. O.
	18	59	ii. *t*.

July (con.).

d	h	m	
20	21	28	ii. T.
21	2	54	i. *t*.
	5	7	i. T.
	22	39	i. *e*.
	23	27	iii. *t*.
22	1	29	iii. T.
	2	15	i. O.
	11	18	ii. *e*.
	14	6	ii. *o*.
	16	34	ii. O.
	21	22	i. *t*.
	23	35	i. T.
23	17	8	i. *e*.
	20	43	i. O.
24	8	16	ii. *t*.
	10	45	ii. T.
	15	50	i. *t*.
	18	3	i. T.
25	7	27	iii. *e*.
	9	49	iii. E.
	11	36	i. *e*.
	13	13	iii. *o*.
	15	11	i. O.
	15	14	iii. O.
26	0	36	ii. *e*.
	3	6	ii. E.
	3	22	ii. *o*.
	5	50	ii. O.
	10	19	i. *t*.
	12	31	i. T.
27	6	5	i. *e*.
	9	39	i. O.
	21	33	ii. *t*.
28	0	1	ii. T.
	4	47	i. *t*.
	6	59	i. T.
29	0	33	i. *e*.
	3	23	iii. *t*.
	4	7	i. O.
	5	22	iii. T.
	13	53	ii. *e*.
	16	23	ii. E.
	16	38	ii. *o*.
	19	5	ii. O.
	23	14	i. *t*.
30	1	27	i. T.
	19	2	i. *e*.
	22	35	i. O.
31	10	49	ii. *t*.
	13	16	ii. T.
	17	42	i. *t*.
	19	55	i. T.

August.

d	h	m	
1	11	28	iii. *e*.
	13	31	i. *e*.
	13	49	iii. E.
	17	3	i. O.
	17	7	iii. *o*.

Aug. (con.).

d	h	m	
1	19	4	iii. O.
2	3	11	ii. *e.*
	5	41	ii. E.
	5	53	ii. *o.*
	8	20	ii. O.
	12	10	i. *t.*
	14	23	i. T.
3	7	59	i. *e.*
	11	31	i. O.
4	0	5	ii. *t.*
	2	32	ii. T.
	6	38	i. *t.*
	8	50	i. T.
5	2	28	i. *e.*
	5	58	i. O.
	7	14	iii. *t.*
	9	9	iii. T.
	16	29	ii. *e.*
	18	58	ii. E.
	19	8	ii. *o.*
	21	34	ii. O.
6	1	6	i. *t.*
	3	18	i. T.
	20	56	i. *e.*
7	0	26	i. O.
	13	19	ii. *t.*
	15	46	ii. T.
	19	33	i. *t.*
	21	45	i. T.
8	15	25	i. *e.*
	15	30	iii. *e.*
	17	49	iii. E.
	18	54	i. O.
	20	56	iii. *o.*
	22	50	iii. O.
9	5	46	ii. *e.*
	8	16	ii. E.
	8	22	ii. *o.*
	10	48	ii. O.
	14	1	i. *t.*
	16	13	i. T.
10	9	53	i. *e.*
	13	21	i. O.
11	2	34	ii. *t.*
	5	0	ii. T.
	8	28	i. *t.*
	10	40	i. T.
12	4	22	i. *e.*
	7	49	i. O.
	11	1	iii. *t.*
	12	53	iii. T.
	19	4	ii. *e.*
	21	33	ii. E.
	21	35	ii. *o.*
13	0	1	ii. O.
	2	56	i. *t.*
	5	8	i. T.
	22	50	i. *e.*
14	2	16	i. O.
	15	48	ii. *t.*
	18	14	ii. T.

Aug. (con.).

d	h	m	
14	21	23	i. *t.*
	23	35	i. T.
15	17	19	i. *e.*
	19	31	iii. *e.*
	20	44	i. O.
	21	49	iii. E.
16	0	41	iii. *o.*
	2	31	iii. O.
	8	21	ii. *e.*
	13	13	ii. O.
	15	50	i. *t.*
	18	2	i. T.
17	11	47	i. *e.*
	15	11	i. O.
18	5	1	ii. *t.*
	7	27	ii. T.
	10	17	i. *t.*
	12	29	i. T.
19	6	16	i. *e.*
	9	38	i. O.
	14	42	iii. *t.*
	16	30	iii. T.
	21	39	ii. *e.*
20	2	25	ii. O.
	4	44	i. *t.*
	6	56	i. T.
21	0	45	i. *e.*
	4	5	i. O.
	18	14	ii. *t.*
	20	39	ii. T.
	23	11	i. *t.*
22	1	23	i. T.
	19	13	i. *e.*
	22	32	i. O.
	23	33	iii. *e.*
23	1	49	iii. E.
	4	21	iii. *o.*
	6	8	iii. O.
	10	57	ii. *e.*
	15	36	ii. O.
	17	38	i. *t.*
	19	50	i. T.
24	13	42	i. *e.*
	16	59	i. O.
25	7	26	ii. *t.*
	9	52	ii. T.
	12	5	i. *t.*
	14	17	i. T.
26	8	10	i. *e.*
	11	26	i. O.
	18	18	iii. *t.*
	20	4	iii. T.
27	0	14	ii. *e.*
	4	47	ii. O.
	6	32	i. *t.*
	8	44	i. T.
28	2	39	i. *e.*
	5	53	i. O.
	20	38	ii. *t.*
	23	3	ii. T.
29	0	59	i. *t.*

Aug. (con.).

d	h	m	
29	3	11	i. T.
	21	8	i. *e.*
30	0	20	i. O.
	3	35	iii. *e.*
	5	49	iii. E.
	7	54	iii. *o.*
	9	39	iii. O.
	13	32	ii. *e.*
	17	58	ii. O.
	19	25	i. *t.*
	21	37	i. T.
31	15	36	i. *e.*
	18	46	i. O.

September.

d	h	m	
1	9	49	ii. *t.*
	12	14	ii. T.
	13	52	i. *t.*
	16	4	i. T.
2	10	5	i. *e.*
	13	13	i. O.
	21	50	iii. *t.*
	23	34	iii. T.
3	2	49	ii. *e.*
	7	7	ii. O.
	8	18	i. *t.*
	10	30	i. T.
4	4	34	i. *e.*
	7	40	i. O.
	22	59	ii. *t.*
5	1	24	ii. T.
	2	45	i. *t.*
	4	57	i. T.
	23	2	i. *e.*
6	2	6	i. O.
	7	36	iii. *e.*
	9	49	iii. E.
	11	23	iii. *o.*
	13	7	iii. O.
	16	7	ii. *e.*
	20	16	ii. O.
	21	11	i. *t.*
	23	23	i. T.
7	17	31	i. *e.*
	20	33	i. O.
8	12	10	ii. *t.*
	14	35	ii. T.
	15	38	i. *t.*
	17	50	i. T.
9	12	0	i. *e.*
	14	59	i. O.
10	1	16	iii. *t.*
	3	0	iii. T.
	5	24	ii. *e.*
	9	25	ii. O.
	10	4	i. *t.*
	12	16	i. T.
11	6	28	i. *e.*
	9	26	i. O.

Sept. (con.).

d	h	m	
12	1	20	ii. *t.*
	3	45	ii. T.
	4	30	i. *t.*
	6	42	i. T.
13	0	57	i. *e.*
	3	52	i. O.
	11	38	iii. *e.*
	13	49	iii. E.
	14	48	iii. *o.*
	16	31	iii. O.
	18	42	ii. *e.*
	22	33	ii. O.
	22	57	i. *t.*
14	1	8	i. T.
	19	25	i. *e.*
	22	18	i. O.
15	14	30	ii. *t.*
	16	54	ii. T.
	17	23	i. *t.*
	19	35	i. T.
16	13	54	i. *e.*
	16	45	i. O.
17	4	39	iii. *t.*
	6	23	iii. T.
	7	59	ii. *e.*
	11	41	ii. O.
	11	49	i. *t.*
	14	1	i. T.
18	8	23	i. *e.*
	11	11	i. O.
19	3	38	ii. *t.*
	6	3	ii. T.
	6	15	i. *t.*
	8	27	i. T.
20	2	52	i. *e.*
	5	37	i. O.
	15	39	iii. *e.*
	17	49	iii. E.
	18	8	iii. *o.*
	19	52	iii. O.
	21	17	ii. *e.*
21	0	41	i. *t.*
	0	48	ii. O.
	2	53	i. T.
	21	20	i. *e.*
22	0	3	i. O.
	16	47	ii. *t.*
	19	7	i. *t.*
	19	12	ii. T.
	21	19	i. T.
23	15	49	i. *e.*
	18	29	i. O.
24	7	58	iii. *t.*
	9	42	iii. T.
	10	35	ii. *e.*
	13	33	i. *t.*
	13	56	ii. O.
	15	44	i. T.
25	10	18	i. *e.*
	12	55	i. O.
26	5	55	ii. *t.*

Sept. (con.).

d	h	m	
26	7	59	i. *t.*
	8	20	ii. T.
	10	10	i. T.
27	4	46	i. *e.*
	7	21	i. O.
	19	42	iii. *e.*
	23	11	iii. O.
	23	51	ii. *e.*
28	2	24	i. *t.*
	3	3	ii. O.
	4	36	i. *t.*
	23	15	i. *e.*
29	1	47	i. O.
	19	4	ii. *t.*
	20	50	i. *t.*
	21	29	ii. T.
	23	2	i. T.
30	17	44	i. *e.*
	20	13	i. O.

October.

d	h	m	
1	11	13	iii. *t.*
	13	0	iii. T.
	13	9	ii. *e.*
	15	16	i. *t.*
	16	9	ii. O.
	17	28	i. T.
2	12	13	i. *e.*
	14	39	i. O.
3	8	11	ii. *t.*
	9	42	i. *t.*
	10	36	ii. T.
	11	54	i. T.
4	6	41	i. *e.*
	9	5	i. O.
	23	44	iii. *e.*
5	2	26	ii. *e.*
	2	29	iii. O.
	4	8	i. *t.*
	5	16	ii. O.
	6	20	i. T.
6	1	10	i. *e.*
	3	31	i. O.
	21	19	ii. *t.*
	22	34	i. *t.*
	23	45	ii. T.
7	0	45	i. T.
	19	39	i. *e.*
	21	57	i. O.
8	14	27	iii. *t.*
	15	44	ii. *e.*
	16	16	iii. T.
	16	59	i. *t.*
	18	22	ii. O.
	19	11	i. T.
9	14	8	i. *e.*
	16	23	i. O.
10	10	27	ii. *t.*
	11	25	i. *t.*

Oct. (con.).

d	h	m	
10	12	53	ii. T.
	13	37	i. T.
11	8	37	i. *e.*
	10	49	i. O.
12	3	47	iii. *e.*
	5	3	ii. *o.*
	5	51	i. *t.*
	5	52	iii. E.
	7	29	ii. O.
	8	3	i. T.
13	3	3	i. *o.*
	5	15	i. E.
	23	35	ii. *t.*
14	0	16	i. *t.*
	2	1	ii. T.
	2	29	i. T.
	21	29	i. *o.*
	23	44	i. E.
15	17	40	iii. *t.*
	18	9	ii. *o.*
	18	42	i. *t.*
	19	33	iii. T.
	20	45	ii. E.
	20	55	i. T.
16	15	55	i. *o.*
	18	12	i. E.
17	12	42	ii. *t.*
	13	8	i. *t.*
	15	9	ii. T.
	15	20	i. T.
18	10	21	i. *o.*
	12	41	i. E.
19	7	8	iii. *o.*
	7	15	ii. *o.*
	7	34	i. *t.*
	9	46	i. T.
	9	53	iii. E.
	10	3	ii. E.
20	4	47	i. *o.*
	7	10	i. E.
21	1	51	ii. *t.*
	2	0	i. *t.*
	4	12	i. T.
	4	18	ii. T.
	23	13	i. *o.*
22	1	39	i. E.
	20	21	ii. *o.*
	20	26	i. *t.*
	20	54	iii. *t.*
	22	38	i. T.
	22	51	iii. T.
	23	20	ii. E.
23	17	39	i. *o.*
	20	8	i. E.
24	14	52	i. *t.*
	14	58	ii. *t.*
	17	4	i. T.
	17	26	ii. T.
25	12	5	i. *o.*
	14	36	i. E.
26	9	18	i. *t.*

Oct. (con.).

d	h	m	
26	9	28	ii. *o.*
	10	23	iii. *o.*
	11	30	i. T.
	12	38	ii. E.
	13	54	iii. E.
27	6	31	i. *o.*
	9	5	i. E.
28	3	44	i. *t.*
	4	7	ii. *t.*
	5	56	i. T.
	6	35	ii. T.
29	0	58	i. *o.*
	3	34	i. E.
	22	10	i. *t.*
	22	35	ii. *o.*
30	0	9	iii. *t.*
	0	22	i. T.
	1	56	ii. E.
	2	10	iii. T.
	19	24	i. *o.*
	22	3	i. E.
31	16	36	i. *t.*
	17	15	ii. *t.*
	18	48	i. T.
	19	44	ii. T.

November.

d	h	m	
1	13	50	i. *o.*
	16	32	i. E.
2	11	12	i. *t.*
	11	42	ii. *o.*
	13	14	i. T.
	13	39	iii. *o.*
	15	13	ii. E.
	15	42	iii. O.
	15	53	iii. *e.*
	17	55	iii. E.
3	8	16	i. *o.*
	11	1	i. E.
4	5	28	i. *t.*
	6	25	ii. *t.*
	7	41	i. T.
	8	54	ii. T.
5	2	43	i. *o.*
	5	30	i. E.
	23	54	i. *t.*
6	0	50	ii. *o.*

Nov. (con.).

d	h	m	
6	2	7	i. T.
	3	28	iii. *t.*
	4	31	ii. E.
	5	33	iii. T.
	21	9	i. *o.*
	23	58	i. E.
7	18	20	i. *t.*
	19	34	ii. *t.*
	20	33	i. T.
	22	3	ii. T.
8	15	35	i. *o.*
	18	27	i. E.
9	12	47	i. *t.*
	13	58	ii. *o.*
	15	0	i. T.
	16	59	iii. *o.*
	17	49	ii. E.
	19	7	iii. O.
	19	56	iii. *e.*
	21	56	iii. E.
10	10	2	i. *o.*
	12	56	i. E.
11	7	13	i. *t.*
	8	44	ii. *t.*
	9	26	i. T.
	11	14	ii. T.
12	4	28	i. *o.*
	7	25	i. E.
13	1	40	i. *t.*
	3	7	ii. *o.*
	3	53	i. T.
	6	49	iii. *t.*
	7	7	ii. E.
	8	59	iii. T.
	22	55	i. *o.*
14	1	54	i. E.
	20	6	i. *t.*
	21	54	ii. *t.*
	22	19	i. T.
15	0	24	ii. T.
	17	22	i. *o.*
	20	23	i. E.
16	14	33	i. *t.*
	16	16	ii. *o.*
	16	46	i. T.
	20	23	iii. *o.*
	20	24	ii. E.
	22	35	iii. O.
	23	59	iii. *e.*

Nov. (con.).

d	h	m	
17	1	58	iii. E.
	11	49	i. *o.*
	14	52	i. E.
18	9	0	i. *t.*
	11	5	ii. *t.*
	11	13	i. T.
	13	36	ii. T.
19	6	16	i. *o.*
	9	21	i. E.
20	3	26	i. *t.*
	5	25	ii. *o.*
	5	40	i. T.
	9	42	ii. E.
	10	15	iii. *t.*
	12	29	iii. T.
21	0	43	i. *o*
	3	50	i. E.
	21	53	i. *t.*
22	0	7	i. T.
	0	16	ii. *t.*
	2	47	ii. T.
	19	10	i. *o.*
	22	19	i. E.
23	16	20	i. *t.*
	18	33	i. T.
	18	36	ii. *o.*
	23	0	ii. E.
	23	52	iii. *o.*
24	2	8	iii. O.
	4	2	iii. *e.*
	5	59	iii. E.
	13	37	i. *o.*
	16	48	i. E.
25	10	47	i. *t.*
	13	0	i. T.
	13	28	ii. *t.*
	16	0	ii. T.
26	8	4	i. *o.*
	11	17	i. E.
27	5	14	i. *t.*
	7	28	i. T.
	7	47	ii. *o.*
	12	18	ii. E.
	13	45	iii. *t.*
	16	3	iii. T.
28	2	31	i. *o.*
	5	46	i. E.
	23	41	i. *t.*

Nov. (con.).

d	h	m	
29	1	55	i. T.
	2	40	ii. *t.*
	5	12	ii. T.
	20	59	i. *o.*
30	0	15	i. E.
	18	9	i. *t.*
	20	22	i. T.
	20	58	ii. *o.*

December.

d	h	m	
1	1	36	ii. E.
	3	26	iii. *o.*
	5	45	iii. O.
	8	5	iii. *e.*
	10	2	iii. E.
	15	26	i. *o.*
	18	43	i. E.
2	12	36	i. *t.*
	14	49	i. T.
	15	54	ii. *t.*
	18	27	ii. T.
3	9	54	i. *o.*
	13	12	i. E.
4	7	3	i. *t.*
	9	17	i. T.
	10	11	ii. *o.*
	14	54	ii. E.
	17	21	iii. *t.*
	19	41	iii. T.
5	4	21	i. *o.*
	7	41	i. E.
6	1	31	i. *t.*
	3	45	i. T.
	5	8	ii. *t.*
	7	41	ii. T.
	22	49	i. *o.*
7	2	10	i. E.
	19	59	i. *t.*
	22	12	i. T.
	23	24	ii. *o.*
8	4	12	ii. E.
	7	5	iii. *o.*
	9	27	iii. O.
	12	8	iii. *e.*
	14	3	iii. E.
	17	17	i. *o.*
	20	39	i. E.

Dec. (con.).

d	h	m	
9	14	26	i. *t.*
	16	40	i. T.
	18	23	ii. *t.*
	20	56	ii. T.
10	11	44	i. *o.*
	15	8	i. E.
11	8	54	i. *t.*
	11	8	i. T.
	12	37	ii. *o.*
	17	31	ii. E.
	21	2	iii. *t.*
	23	26	iii. T.
12	6	12	i. *o.*
	9	37	i. E.
13	3	22	i. *t.*
	5	36	i. T.
	7	37	ii. *t.*
	10	10	ii. T.
14	0	40	i. *o.*
	4	6	i. E.
	21	50	i. *t.*
15	0	3	i. T.
	1	51	ii. *o.*
	6	49	ii. E.
	10	48	iii. *o.*
	13	13	iii. O.
	16	11	iii. *e.*
	18	4	iii. E.
	19	8	i. *o.*
	22	35	i. E.
16	16	18	i. *t.*
	18	32	i. T.
	20	53	ii. *t.*
	23	27	ii. T.
17	13	36	i. *o.*
	17	4	i. E.
18	10	45	i. *t.*
	12	59	i. T.
	15	6	ii. *o.*
	20	7	ii. E.
19	0	49	iii. *t.*
	3	15	iii. T.
	8	5	i. *o.*
	11	33	i. E.
20	5	14	i. *t.*
	7	23	i. T.
	10	9	ii. *t.*
	12	43	ii. T.

Dec. (con.).

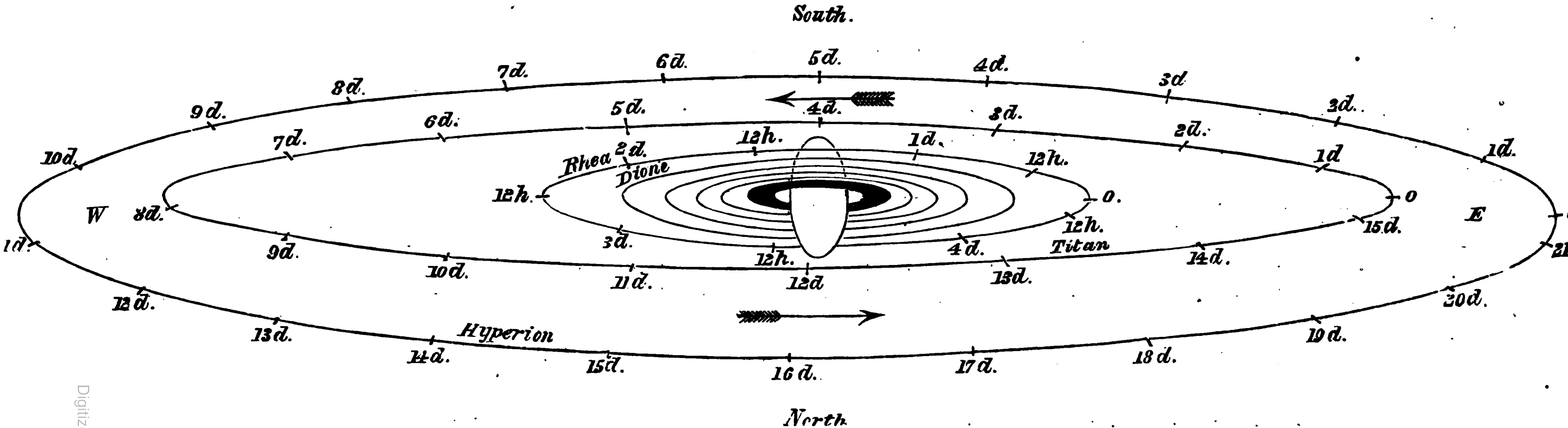
d	h	m	
21	2	33	i. *o.*
	6	2	i. E.
	23	42	i. *t.*
22	1	56	i. T.
	4	22	ii. *o.*
	9	25	ii. E.
	14	38	iii. *o.*
	17	4	iii. O.
	20	14	iii. *c.*
	21	1	i. *o.*
	22	6	iii. E.
23	0	31	i. E.
	18	10	i. *t.*
	20	24	i. T.
	23	26	ii. *t.*
24	2	0	ii. T.
	15	30	i. *o.*
	19	0	i. E.
25	12	39	i. *t.*
	14	52	i. T.
	17	38	ii. *o.*
	22	43	ii. E.
26	4	42	iii. *t.*
	7	9	iii. T.
	9	58	i. *o.*
	13	29	i. E.
27	7	7	i. *t.*
	9	21	i. T.
	12	43	ii. *t.*
	15	17	ii. T.
28	4	27	i. *o.*
	7	58	i. E.
29	1	36	i. *t.*
	3	50	i. T.
	6	55	ii. *o.*
	12	2	ii. E.
	18	32	iii. *o.*
	21	0	iii. O.
	22	56	i. *o.*
30	0	16	iii. *e.*
	2	7	iii. E.
	2	27	i. E.
	20	5	i. *t.*
	22	19	i. T.
31	2	1	ii. *t.*
	4	36	ii. T.
	17	24	i. *o.*
	20	56	i. E.

Satellites I. and II. *reappear* at eclipse on the *f* side in January and February, and from October 13 to the end of the year. They *disappear* on the *p* side from April till October 12.

Satellite III. *disappears* and *reappears f* in January and till February 4, on which date it is eclipsed a few seconds only after reappearing from occultation. After this the disappearances at eclipse become invisible. On April 30 similarly the satellite emerges from eclipse for 6 minutes only and is then occulted. On and after this date both disappearance and reappearance at eclipse are *p* till September 20; *disappearance p* only on September 27 and October 4; *disappearance p* and *reappearance f* on October 12; *reappearance f* only on October 26; and both *disappearance* and *reappearance f* to the end of the year.

Satellite IV. both *disappears* and *reappears* on the *f* side in January and February; and on the *p* side in April.

APPARENT ORBITS OF THE SEVEN INNER SATELLITES OF SATURN, as seen in an Inverting Telescope.



The arrows in the diagram show the direction of the motion of the Satellites. The figures indicate the interval from the time of last East Elongation.

(The Vertical Scale is twice the Horizontal.)

SATELLITES OF SATURN.

East Elongations.

MIMAS.

January.		February.		March.		April.		May.		June.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
1	1·7	1	4·4	1	10·0	1	12·8	1	16·5	1	18·8	16	17·9
2	0·3	2	3·1	2	8·6	2	11·4	2	15·1	2	17·4	17	16·5
2	22·9	3	1·7	3	7·3	3	10·0	3	13·7	3	16·1	18	15·4
3	21·5	4	0·3	4	5·9	4	8·6	4	12·3	4	14·7	19	14·0
4	20·2	4	22·9	5	4·5	5	7·2	5	11·0	5	13·3	20	12·6
5	18·8	5	21·6	6	3·1	6	5·8	6	9·6	6	12·0	21	11·1
6	17·4	6	20·2	7	1·7	7	4·5	7	8·2	7	10·6	22	9·7
7	16·1	7	18·9	8	0·3	8	3·1	8	6·8	8	9·2	23	8·3
8	14·7	8	17·6	8	22·9	9	1·7	9	5·4	9	7·8	24	6·9
9	13·4	9	16·2	9	21·6	10	0·3	10	4·1	10	6·4	25	5·5
10	12·0	10	14·9	10	20·3	10	22·9	11	2·7	11	5·1	26	4·1
11	10·6	11	13·6	11	18·9	11	21·5	12	1·3	12	3·7	27	2·7
12	9·2	12	12·2	12	17·5	12	20·2	13	0·0			28	1·3
13	7·8	13	10·8	13	16·2	13	18·8	13	22·6			29	0·0
14	6·5	14	9·3	14	14·8	14	17·4	14	21·2			29	22·6
15	5·1	15	7·9	15	13·4	15	16·0	15	19·8			30	21·2
16	3·7	16	6·5	16	12·1	16	14·6	16	18·4			31	19·8
17	2·3	17	5·2	17	10·8	17	13·3	17	17·0				
18	0·9	18	3·8	18	9·6	18	12·0	18	15·6				
18	23·6	19	2·4	19	8·2	19	10·6	19	14·2				
19	22·3	20	1·1	20	6·8	20	9·2	20	12·9				
20	21·0	20	23·7	21	5·4	21	7·8	21	11·5				
21	19·5	21	22·3	22	4·0	22	6·4	22	10·1				
22	18·1	22	20·9	23	2·6	23	5·0	23	8·7				
23	16·8	23	19·6	24	1·2	24	3·6	24	7·4				
24	15·4	24	18·2	24	23·8	25	2·2	25	6·0				
25	14·1	25	16·8	25	22·5	26	0·8	26	4·6				
26	12·7	26	15·5	26	21·1	26	23·4	27	3·2				
27	11·3	27	14·1	27	19·8	27	22·0	28	1·8				
28	10·0	28	12·8	28	18·4	28	20·7	29	0·4				
29	8·6	29	11·4	29	17·0	29	19·3	29	23·0				
30	7·2			30	15·6	30	17·9	30	21·6				
31	5·8			31	14·2			31	20·2				

ENCELADUS.

January.		February.		March.		April.		May.		June.		December.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
1	1·1	1	13·3	1	7·8	1	20·2	1	23·6	1	3·0	12	18·3
2	10·0	2	22·2	2	16·7	3	5·1	3	8·5	2	11·9	14	3·2
3	18·8	4	7·1	4	1·6	4	14·0	4	17·4	3	20·8	15	12·1
5	3·7	5	16·0	5	10·5	5	22·9	6	2·3	5	5·6	16	21·0
6	12·6	7	0·8	6	19·3	7	7·8	7	11·1	6	14·5	18	5·9
7	21·5	8	9·7	8	4·2	8	16·6	8	20·0	7	23·4	19	14·7
9	6·4	9	18·6	9	13·1	10	1·5	10	4·9	9	8·3	20	23·6
10	15·3	11	3·6	10	22·0	11	10·4	11	13·8	10	17·2	22	8·5
12	0·1	12	12·4	12	6·9	12	19·3	12	22·7	12	2·0	23	17·4
13	9·0	13	21·3	13	15·7	14	4·2	14	7·5	13	10·9	25	2·2
14	17·9	15	6·1	15	0·7	15	13·0	15	16·4	14	19·8	26	11·0
16	2·8	16	15·0	16	9·7	16	21·9	17	1·3	16	4·7	27	19·9
17	11·6	17	23·9	17	18·6	18	6·8	18	10·2	17	13·6	29	4·8
18	20·5	19	8·8	19	3·4	19	15·7	19	19·1			30	13·7
20	5·4	20	17·6	20	12·3	21	0·6	21	4·0			31	22·6
21	14·3	22	2·5	21	21·2	22	9·4	22	12·8				
22	23·2	23	11·4	23	6·1	23	18·3	23	21·7				
24	8·0	24	20·4	24	14·9	25	3·2	25	6·6				
25	16·9	26	5·2	25	23·8	26	12·1	26	15·5				
27	1·8	27	14·1	27	8·7	27	21·0	28	0·4				
28	10·8	28	22·9	28	17·6	29	5·8	29	9·2				
29	19·6			30	2·5	30	14·7	30	18·1				
31	4·5			31	11·4								

East Elongations.

Tethys.

January.	February.	March.	April.	May.	June.	December.
d h	d h	d h	d h	d h	d h	d h
3 0·6	2 5·4	1 12·9	2 15·1	2 19·9	2 0·6	7 21·5
4 21·9	4 2·7	3 10·2	4 12·4	4 17·2	3 21·9	9 18·8
6 19·2	6 0·0	5 7·5	6 9·7	6 14·5	5 19·2	11 16·2
8 16·5	7 21·3	7 4·8	8 7·0	8 11·8	7 16·5	13 13·5
10 13·8	9 18·6	9 2·1	10 4·3	10 9·1	9 13·8	15 10·8
12 11·1	11 15·9	10 23·4	12 1·6	12 6·3	11 11·1	17 8·0
14 8·4	13 13·2	12 20·7	13 22·9	14 3·8	13 8·4	19 5·3
16 5·7	15 10·5	14 18·1	15 20·2	16 0·9	15 5·7	21 2·7
18 3·0	17 7·8	16 15·4	17 17·5	17 22·2	17 3·0	23 0·0
20 0·3	19 5·1	18 12·7	19 14·8	19 19·5	19 0 3	24 21·3
21 21·6	21 2·4	20 10·0	21 12·1	21 16·8	20 21·6	26 18·6
23 18·9	22 23·7	22 7·3	23 9·6	23 14·1	22 18·9	28 15·9
25 16·2	24 21·0	24 4·6	25 6·7	25 11·4	24 16·2	30 13·2
27 13·5	26 18·3	26 1·9	27 4·0	27 8·7		
29 10·8	28 15·6	27 23·2	29 1·3	29 6·0		
31 8·1		29 20·5	30 22·6	31 3·3		
		31 17·8				

Dione.

January.	February.	March.	April.	May.	June.	December.
d h	d h	d h	d h	d h	d h	d h
2 8·0	1 10·5	2 12·9	1 15·4	1 17·7	3 13·7	9 13·2
5 1·7	4 4·1	5 6·6	4 9·1	4 11·4	6 7·3	12 6·9
7 19·3	6 21·8	8 0·3	7 2·7	7 5·0	9 1·0	15 0·6
10 13·0	9 15·5	10 18·0	9 20·4	9 22·7	11 18·7	17 18·3
13 6·7	12 9·2	13 11·7	12 14·0	12 16·4	14 12·3	20 11·9
16 0·4	15 2·9	16 5·4	15 7·7	15 10·0	17 6·0	23 5·6
18 18·0	17 20·5	18 23·0	18 1·4	18 3·7	19 23·6	25 23·3
21 11·7	20 14·2	21 16·7	21 19·0	20 21·4		28 17·0
24 5·4	23 7·9	24 10·4	23 12·7	23 15·0		31 10·7
26 23·1	26 1·6	27 4·1	26 6·4	26 8·7		
29 16·8	28 19·3	29 21·7	29 0·0	29 2·4		
				31 10·0		

Rhea.

January.	February.	March.	April.	May.	June.	December.
d h	d h	d h	d h	d h	d h	d h
1 7·9	1 22·5	4 13·2	5 3·8	2 6·0	2 20·3	9 19·3
5 20·3	6 10·9	9 1·6	9 16·2	6 18·4	7 8·7	14 7·7
10 8·6	10 23·3	13 13·9	14 4·6	11 6·7	11 21·1	18 20·0
14 21·0	15 11·7	18 2·3	18 16·9	15 19·1	16 9·4	23 8·4
19 9·4	20 0·0	22 14·7	23 5·3	20 7·5		27 20·8
23 21·8	24 12·4	27 3·1	27 17·7	24 19·8		
28 10·2	29 0·8	31 15·4		29 8·0		

Titan.

January.	February.	March.	April.	May.	December.	
d h	d h	d h	d h	d h	d h	
3 23·9	4 21·2	7 17·5	8 10·8	10 6·9	20 6·1	......
19 22·6	20 19·5	23 13·3	24 8·5	26 5·3	......	......

East Elongations.

HYPERION.

January.	February.	March.	April.	May.	November.	December.	
d h	d h	d h	d h	d h	d h	d h	
12 0·1	2 6·0	15 15·7	5 20·3	18 7·6	5 5·6	18 1·6	......
	23 11·1		27 1·5		26 14·8	......	......

IAPETUS.

	d h	d h	d h	d h	d h
Inf. Conjunction ...	Feb. 19 13·9,	May 7 22·5,	July 26 18·6,	Oct. 15 21·8.	
W. Elongation	Mar. 9 6·0,	May 26 17·6,	Aug. 14 20·5,	Nov. 4 14·2.	
Sup. Conjunction ...	Jan. 11 6·6,	Mar. 29 16·5,	June 16 14·0,	Nov. 25 17·2.	
E. Elongation	Jan. 31 23·2,	Apr. 19 4·1,	July 7 10·1,	Sep. 27 4·1,	Dec. 16 21·1.

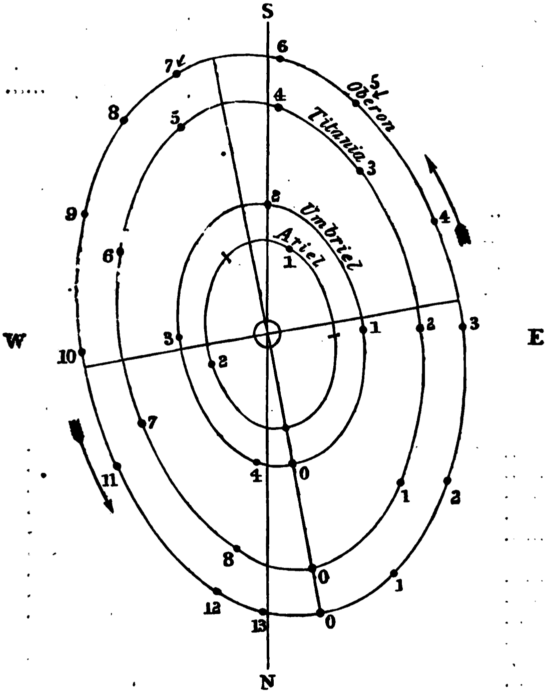
Apparent Elements of Saturn's Rings.

Greenwich Mean Noon.	Position-angle of Minor Axis.	Outer Ring. Maj. Axis.	Outer Ring. Min. Axis.	Latitude above Plane of Ring. Earth.	Latitude above Plane of Ring. Sun.
	° ′	″	″	° ′	° ′
Jan. 1	355 54·5	40·68	2·59	3 39·2 N.	0 56·4 N.
21	355 54·4	42·09	2·60	3 32·2 N.	1 16·1 N.
Feb. 10	355 50·6	43·29	2·35	3 6·7 N.	1 34·5 N.
Mar. 1	355 43·9	44·06	1·88	2 27·5 N.	1 52·8 N.
21	355 35·9	44·23	1·32	1 42·7 N.	2 11·1 N.
April 10	355 28·3	43·77	0·78	1 1·8 N.	2 29·4 N.
30	355 21·8	42·78	0·40	0 32·4 N.	2 47·6 N.
May 20	355 20·0	41·47	0·27	0 22·2 N.	3 5·8 N.
June 9	355 20·7	40·05	0·36	0 30·6 N.	3 23·9 N.
29	355 24·7	38·69	0·65	0 57·4 N.	3 42·1 N.
July 19	355 31·7	37·51	1·09	1 40·0 N.	4 0·1 N.
Aug. 8	355 41·3	36·58	1·64	2 34·7 N.	4 18·1 N.
28	355 52·8	35·95	2·27	3 37·6 N.	4 36·1 N.
Sept. 17	356 5·7	35·64	2·95	4 44·6 N.	4 53·9 N.
Oct. 7	356 19·3	35·67	3·64	5 51·6 N.	5 11·8 N.
27	356 32·8	36·03	4·33	6 54·6 N.	5 29·6 N.
Nov. 16	356 45·3	36·72	5·00	7 49·4 N.	5 47·3 N.
Dec. 6	356 55·8	37·72	5·69	8 32·2 N.	6 4·9 N.
26	357 3·3	38·96	6·09	8 59·5 N.	6 22·5 N.
31	357 4·6	39·30	6·19	9 3·7 N.	6 26·9 N.

The corrections to reduce to Mr. Marth's Ephemeris will be given in the Monthly numbers as soon as possible.

TABLE OF BESSEL'S MEAN REFRACTIONS.

Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.	Z. D.	Mean Refraction.
°	′ ″	°	′ ″	°	′ ″	°	′ ″
0	0·0	56	1 25·4	68	2 21·9	78	4 25·0
10	10·2	58	1 32·1	69	2 29·3	79	4 48·5
20	21·0	60	1 39·7	70	2 37·3	80	5 16·2
30	33·3	61	1 43·8	71	2 46·1	81	5 49·3
35	40·4	62	1 48·2	72	2 55·8	82	6 29·6
40	48·4	63	1 52·8	73	3 6·6	83	7 19·7
45	57·7	64	1 57·8	74	3 18·6	84	8 23·3
50	1 8·7	65	2 3·2	75	3 32·1	85	9 46·5
52	1 13·8	66	2 8·9	76	3 47·4		
54	1 19·3	67	2 15·2	77	4 4·9		



Apparent Orbits of the Satellites of Uranus, as seen in an inverting telescope.

The small circle in the above diagram represents the planet, the arrows show the direction of motion, and the figures indicate the interval from the time of last North Elongation.

North Elongations.

ARIEL.

January.		February.		March.		April.		May.		June.		July.	
d	h	d	h	d	h	d	h	d	h	d	h	d	h
17	11·8	1	14·7	2	20·5	2	2·4	2	8·3	1	14·2	1	20·0
20	0·3	4	3·2	5	9·0	4	14·9	4	20·8	4	2·7	4	8·5
22	12·8	6	15·7	7	21·5	7	3·4	7	9·3	6	15·2	6	21·0
25	1·2	9	4·2	10	10·0	9	15·9	9	21·7	9	3·7	9	9·5
		11	16·6	12	22·5	12	4·4	12	10·2	11	16·1	11	22·0
		14	5·1	15	11·0	14	16·9	14	22·7	14	4·6	14	10·5
		16	17·6	17	23·5	17	5·4	17	11·2	16	17·1	16	23·0
		19	6·1	20	12·0	19	17·9	19	23·7	19	5·6		
		21	18·5	23	0·5	22	6·4	22	12·2	21	18·1		
		24	7·0	25	13·0	24	18·9	25	0·7	24	6·6		
		26	19·5	28	1·4	27	7·3	27	13·2	26	19·1		
		29	8·0	30	13·9	29	19·8	30	1·7	29	7·6		

UMBRIEL.

January.	February.	March.	April.	May.	June.	July.
d h	d h	d h	d h	d h	d h	d h
17 12·5	3 2·3	3 2·5	1 2·8	4 6·5	2 6·7	1 7·0
21 16·0	7 5·8	7 5·9	5 6·3	8 10·0	6 10·2	5 10·5
25 19·4	11 9·2	11 9·4	9 9·7	12 13·4	10 13·7	9 13·9
29 22·9	15 12·7	15 12·9	13 13·2	16 16·9	14 17·2	13 17·3
	19 16·2	19 16·4	17 16·7	20 20·3	18 20·7	17 20·8
	23 19·7	23 19·9	21 20·2	24 23·8	23 0·2	22 0·3
	27 23·1	27 23·4	25 23·6	29 3·3	27 3·6	26 3·8
			30 3·1			30 7·3

TITANIA.

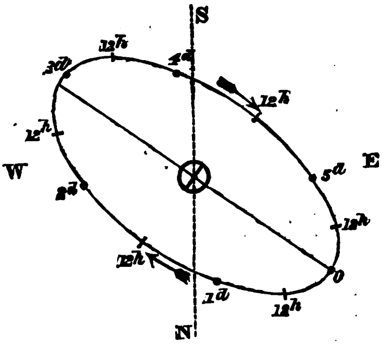
13 12·7	8 15·6	5 18·5	9 14·4	5 17·3	9 13·1	5 16·1
22 5·6	17 8·5	14 11·4	18 7·3	14 10·2	18 6·1	14 9·0
30 22·6	26 1·5	23 4·4	27 0·3	23 3·2	26 23·1	23 2·0
		31 21·4		31 20·2		31 19·0

OBERON.

16 19·4	12 17·8	10 16·1	6 14·5	3 12·9	12 22·5	......
30 6·6	26 4·9	24 3·3	20 1·7	17 0·1	26 9·6	......
				30 11·3		

Position-angle and distance of the apse of Oberon:—January 16, 7°·6, 43″·3; May 30, 9°·1, 45″·6.

SATELLITE OF NEPTUNE.



Apparent Orbit of the Satellite of Neptune, as seen in an inverting telescope. The small circle in the above diagram represents the planet, the arrows show the direction of motion, and the figures indicate the interval from the time of last North-east Elongation.

North-east Elongations.

January.	February.	September.	October.	November.	December.	
d h	d h	d h	d h	d h	d h	
1 12·2	5 18·7	5 6·7	4 16·1	3 1·5	2 10·9	......
7 9·3	11 15·8	11 3·8	10 13·2	8 22·6	8 8·0	......
13 6·4	17 12·9	17 0·8	16 10·3	14 19·7	14 5·1	......
24 3·4	23 10·0	22 21·9	22 7·4	20 16·8	20 2·2	......
25 0·5	29 7·1	28 19·0	28 4·4	26 13·9	25 23·3	......
30 21·6					31 20·4	

Position-angle and distance of the apse:—January 10, 241°·8, 16″·8; September 1, 246°·3, 16″·4; Dec. 27, 244°·3, 16″·9.

EPHEMERIS FOR PHYSICAL OBSERVATIONS OF THE SUN.

Greenwich Mean Noon.				Greenwich Mean Noon.			
1892.	P.	D.	L.	1892.	P.	D.	L.
	° ′	° ′	° ′		′ °	° ′	° ′
Jan. 1	+ 1 56	−3 14	80 50	July 4	− 0 50	+3 30	159 0
6	− 0 31	3 48	15 0	9	+ 1 28	4 0	92 50
11	2 56	4 20	309 9	14	3 42	4 30	26 42
16	5 18	4 50	243 18	19	5 56	4 57	320 31
21	7 37	5 17	177 29	24	8 4	5 23	254 22
26	9 51	5 43	111 39	29	10 9	5 46	188 16
31	11 59	6 5	45 49	Aug. 3	12 9	6 7	122 7
Feb. 5	13 59	6 24	339 59	8	14 2	6 25	56 0
10	15 53	6 41	274 9	13	15 50	6 40	349 54
15	17 38	6 54	208 19	18	17 30	6 53	283 49
20	19 15	7 4	142 28	23	19 4	7 3	217 45
25	20 43	7 11	76 37	28	20 30	7 10	151 43
Mar. 1	22 1	7 15	10 45	Sept. 2	21 47	7 14	85 39
6	23 10	7 15	304 52	7	22 55	7 15	19 37
11	24 9	7 11	238 59	12	23 55	7 13	313 36
16	24 59	7 5	173 5	17	24 46	7 7	247 37
21	25 37	6 55	107 9	22	25 27	6 58	181 36
26	26 6	6 42	41 13	27	25 58	6 47	115 37
31	26 23	6 27	335 15	Oct. 2	26 19	6 32	49 40
Apr. 5	26 30	6 8	269 17	7	26 29	6 14	343 40
10	26 26	5 47	203 17	12	26 28	5 53	277 43
15	26 11	5 23	137 16	17	26 16	5 30	211 46
20	25 44	4 57	71 13	22	25 52	5 4	145 51
25	25 7	4 29	5 9	27	25 17	4 36	79 53
30	24 19	3 59	299 5	Nov. 1	24 30	4 6	13 57
May 5	23 19	3 28	232 59	6	23 30	3 33	308 2
10	22 10	2 55	166 52	11	22 20	3 0	242 7
15	20 49	2 21	100 44	16	20 57	2 24	176 12
20	19 19	1 46	34 36	21	19 23	1 47	110 18
25	17 40	1 10	328 27	26	17 39	1 10	44 24
30	15 53	0 34	262 17	Dec. 1	15 45	+0 32	338 30
June 4	13 59	−0 2	196 6	6	13 42	−0 7	272 37
9	11 55	+0 38	129 57	11	11 33	0 45	206 44
14	9 50	1 14	63 44	16	9 16	1 23	140 52
19	7 37	1 49	357 35	21	6 54	2 1	75 0
24	5 22	2 24	291 22	26	4 30	2 37	9 8
29	− 3 6	+2 57	225 13	31	+ 2 3	−3 12	303 17

The position-angle of the Sun's axis, P, is the position-angle of the N. end of the axis from the N. point of the Sun, read in the direction N., E., S., W. In computing D (the heliographic latitude of the centre of the Sun's disk), the inclination of the Sun's axis to the ecliptic has been assumed to be 82° 45′, and the longitude of the ascending node to be 74° 19′. In computing L (the heliographic longitude of the centre of the disk), the Sun's period of rotation has been assumed to be 25·38 days, and the meridian which passed through the ascending node at the epoch 1854·0 has been taken as the zero meridian.

MEAN PLACES OF VARIABLE STARS FOR 1890.

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
1.	T Cassiopeiæ	0 17 17	+55 11·0	53.	S Hydræ	8 47 50	+ 3 29·0
2.	R Andromedæ	0 18 13	+37 58·1	54.	T Hydræ	8 50 19	− 8 43·3
3.	S Ceti	0 18 28	− 9 56·3	55.	T Cancri	8 50 23	+20 16·1
4.	U Cassiopeiæ	0 40 13	+47 39·3	56.	S Antliæ	9 27 30	−28 8·6
5.	U Cephei	0 52 32	+81 16·9	57.	R Carini	9 29 28	−62 18·1
6.	S Cassiopeiæ	1 11 34	+72 1·9	58.	R Leo. Min.	9 38 59	+35 1·2
7.	S Piscium	1 11 49	+ 8 21·1	59.	R Leonis	9 41 39	+11 56·4
8.	U Piscium	1 17 9	+12 17·4	60.	*l* Carini	9 42 14	−62 0·1
9.	R Sculptoris	1 21 55	−33 6·8	61.	V Leonis	9 53 57	+21 47·2
10.	R Piscium	1 24 58	+ 2 18·8	62.	U Hydræ	10 32 7	−12 48·8
11.	S Arietis	1 58 43	+11 59·9	63.	R Ursæ Maj.	10 36 50	+69 21·2
12.	R Arietis	2 9 51	+24 32·7	64.	V Hydræ	10 46 16	−20 39·9
13.	*o* Ceti	2 13 47	− 3 28·6	65.	W Leonis	10 47 49	+14 18·1
14.	S Persei	2 14 59	+58 5·0	66.	R Crateris	10 55 9	−17 44·0
15.	R Ceti	2 20 24	− 0 40·5	67.	S Leonis	11 5 9	+ 6 3·5
16.	U Ceti	2 28 26	−13 37·8	68.	R Comæ	11 58 37	+19 23·9
17.	T Arietis	2 42 11	+17 3·1	69.	T Virginis	12 8 58	− 5 25·4
18.	Algol	3 1 1	+40 32·0	70.	R Corvi	12 13 56	−18 38·6
19.	R Persei	3 23 3	+35 17·7	71.	Y Virginis	12 28 12	− 3 48·9
20.	λ Tauri	3 54 35	+12 10·8	72.	T Ursæ Maj.	12 31 23	+60 5·6
21.	W Tauri	4 21 45	+15 51·3	73.	R Virginis	12 32 55	+ 7 35·6
22.	R Tauri	4 22 16	+ 9 55·0	74.	S Ursæ Maj.	12 39 7	+61 41·8
23.	S Tauri	4 23 10	+ 9 42·1	75.	U Virginis	12 45 31	+ 6 9·2
24.	R Reticuli	4 32 24	−63 15·4	76.	W Virginis	13 20 21	− 2 48·4
25.	V Tauri	4 45 39	+17 21·1	77.	V Virginis	13 22 7	− 2 36·1
26.	R Orionis	4 53 2	+ 7 57·8	78.	R Hydræ	13 23 42	−22 42·7
27.	R Leporis	4 54 35	−14 58·3	79.	S Virginis	13 27 15	− 6 37·7
28.	R Aurigæ	5 8 24	+53 27·7	80.	W Hydræ	13 42 50	−27 49·1
29.	S Orionis	5 23 35	− 4 46·9	81.	R² Virginis	13 59 3	− 8 40·2
30.	U Orionis	5 49 17	+20 9·3	82.	Z Virginis	14 4 25	−12 47·0
31.	η Geminorum	6 8 14	+22 32·3	83.	X Boötis	14 18 59	+16 49·3
32.	V Monocerotis	6 17 11	− 2 8·5	84.	S Boötis	14 19 12	+54 18·7
33.	T Monocerotis	6 19 17	+ 7 8·8	85.	V Boötis	14 25 20	+39 21·0
34.	R Lyncis	6 52 13	+55 29·1	86.	R Camelopardi	14 25 55	+84 19·9
35.	ζ Geminorum	6 57 35	+20 43·8	87.	R Boötis	14 32 20	+27 12·8
36.	R Geminorum	7 0 43	+22 52·4	88.	U Boötis	14 49 15	+18 8·6
37.	R Canis Min.	7 2 39	+10 11·8	89.	δ Libræ	14 55 6	− 8 4·9
38.	L² Puppis	7 10 12	−44 27·8	90.	T Libræ	15 4 28	−19 36·0
39.	R Canis Maj.	7 14 30	−16 11·2	91.	U Coronæ	15 13 43	+32 2·9
40.	V Geminorum	7 16 59	+13 18·1	92.	S Libræ	15 15 4	−19 59·5
41.	U Monocerotis	7 25 32	− 9 32·8	93.	S Serpentis	15 16 31	+14 42·5
42.	S Canis Min.	7 26 45	+ 8 33·2	94.	S Coronæ	15 16 55	+31 45·7
43.	T Canis Min.	7 27 53	+11 58·8	95.	X Libræ	15 29 52	−20 47·9
44.	U Canis Min.	7 35 22	+ 8 38·2	96.	W Libræ	15 31 42	−15 48·8
45.	S Geminorum	7 36 26	+23 42·6	97.	U Libræ	15 35 39	−20 49·5
46.	T Geminorum	7 42 41	+24 0·5	98.	Z Libræ	15 40 7	−20 47·2
47.	U Geminorum	7 48 34	+22 17·5	99.	V Coronæ	15 45 37	+39 54·1
48.	U Puppis	7 55 42	−12 33·7	100.	R Serpentis	15 45 38	+15 28·1
49.	R Cancri	8 10 30	+12 3·9	101.	R Libræ	15 47 22	−15 54·5
50.	V Cancri	8 15 26	+17 38·0	102.	R² Libræ	15 50 5	−17 58·8
51.	U Cancri	8 29 28	+19 16·5	103.	Z Scorpii	15 59 36	−21 25·9
52.	S Cancri	8 37 39	+19 25·8	104.	R Herculis	16 1 17	+18 40·1

Mean Places of Variable Stars for 1890 (continued).

No.	Star.	R.A.	Dec.	No.	Star.	R.A.	Dec.
		h m s	° ′			h m s	° ′
105.	X Scorpii	16 2 5	−21 13·8	140.	η Aquilæ.........	19 46 52	+ 0 43·4
106.	W Scorpii	16 5 20	−19 51·0	141.	S Sagittæ	19 51 1	+16 20·5
107.	R Scorpii	16 11 5	−22 40·3	142.	S Cygni	20 3 11	+57 40·1
108.	S Scorpii.........	16 11 6	−22 37·2	143.	R Capricorni ...	20 5 8	−14 35·7
109.	W Ophiuchi ...	16 15 29	− 7 26·2	144.	S Aquilæ.........	20 6 34	+15 17·7
110.	V Ophiuchi ...	16 20 37	−12 10·0	145.	W Capricorni...	20 8 0	−22 18·6
111.	U Herculis......	16 20 57	+19 8·4	146.	R Sagittæ	20 9 3	+16 23·6
112.	Y Scorpii	16 23 17	−19 16·2	147.	R Delphini......	20 9 37	+ 8 45·4
113.	T Ophiuchi......	16 27 26	−15 53·8	148.	U Cygni	20 16 12	+47 32·8
114.	S Ophiuchi......	16 27 55	−16 55·7	149.	V Cygni	20 37 45	+47 44·9
115.	W Herculis......	16 31 20	+37 33·9	150.	S Delphini	20 38 1	+16 41·6
116.	R Ursæ Min. ...	16 31 27	+72 30·0	151.	X Cygni	20 39 5	+35 11·4
117.	R Draconis ...	16 32 22	+66 59·2	152.	T Delphini	20 40 16	+15 59·9
118.	S Herculis	16 46 53	+15 7·6	153.	U Capricorni ...	20 42 1	−15 11·3
119.	R Ophiuchi......	17 1 26	−15 56·6	154.	T Aquarii	20 44 8	− 5 33·3
120.	U Ophiuchi......	17 10 56	+ 1 20·0	155.	T Vulpeculæ ...	20 46 48	+27 50·1
121.	X Sagittarii ...	17 40 37	−27 47·2	156.	Y Cygni	20 47 40	+34 14·7
122.	Y Ophiuchi......	17 46 45	− 6 6·9	157.	R Vulpeculæ ...	20 59 30	+23 23·2
123.	W Sagittarii ...	17 58 0	−29 35·1	158.	V Capricorni ...	21 1 12	−24 21·7
124.	T Herculis	18 4 56	+31 0·1	159.	X Capricorni ...	21 2 15	−21 47·5
125.	Y Sagittarii......	18 14 55	−18 54·5	160.	T Cephei.........	21 8 4	+68 2·6
126.	T Serpentis......	18 23 27	+ 6 13·5	161.	T Capricorni ...	21 15 57	−15 37·4
127.	U Sagittarii ...	18 25 25	−19 12·3	162.	Y Capricorni ...	21 28 23	−14 27·6
128.	X Ophiuchi ...	18 33 7	+ 8 43·9	163.	W Cygni.........	21 31 53	+44 53·0
129.	R Scuti	18 41 37	− 5 49·5	164.	S Cephei	21 36 35	+78 7·7
130.	β Lyræ	18 46 1	+33 14·1	165.	U Aquarii	21 57 20	−17 9·4
131.	R Lyræ	18 51 59	+43 48·0	166.	T Pegasi	22 3 31	+12 0·1
132.	R Aquilæ	19 1 4	+ 8 3·8	167.	δ Cephei	22 25 5	+57 51·1
133.	T Sagittarii ...	19 9 53	−17 9·8	168.	R Lacertæ	22 38 23	+41 47·7
134.	R Sagittarii ...	19 10 15	−19 30·2	169.	S Aquarii	22 51 12	−20 55·7
135.	S Sagittarii......	19 12 59	−19 13·5	170.	R Pegasi	23 1 8	+ 9 57·0
136.	U Aquilæ	19 23 26	− 7 16·2	171.	S Pegasi	23 14 59	+ 8 19·0
137.	R Cygni	19 33 52	+49 57·2	172.	R Aquarii	23 38 8	−15 53·7
138.	S Vulpeculæ ...	19 43 53	+27 0·7	173.	V Ceti............	23 52 16	− 9 34·4
139.	χ Cygni	19 46 20	+32 38·1	174.	R Cassiopeiæ ...	23 52 48	+50 46·5

VARIABLES OF SHORT PERIOD NOT OF THE ALGOL TYPE.

(The No. after Star's Name refers to Catalogue on pp. 23–24.)

Note.—The times of minima only are given; the times of maxima may be found by adding the interval printed under the name of the star.

T Monocer. 33.

($7^d\ 22^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
12	5	26	6	11	6
Feb.		June		Oct.	
8	6	22	6	8	6
Mar.		July		Nov.	
6	6	19	6	4	6
Apr.		Aug.		Dec.	
2	6			1	7
29	9	15	6	28	7

ζ Geminor. 35.

($5^d\ 9^h$.)

Jan.		Apr.		Sept.	
d	h	d	h	d	h
8	14	19	3	28	14
18	17	29	6	Oct.	
28	21	May		8	17
Feb.		9	10	18	21
8	1	19	14	29	1
18	4	29	17	Nov.	
28	8	Aug.		8	4
Mar.		18	23	18	8
9	12	29	3	28	12
19	15	Sept.		Dec.	
29	19	8	6	8	15
Apr.		18	10	18	19
8	23			28	23

W Virginis 76.

($8^d\ 5^h$.)

Jan.		Feb.		Mar.	
d	h	d	h	d	h
14	7	17	20	6	2
31	13			23	9

W Virginis 76.

(*continued*).

Apr.		July		Nov.	
d	h	d	h	d	h
9	15	5	0	2	21
26	22	22	6	20	4
May		Aug.		Dec.	
14	4	8	13	7	10
31	11	25	19	24	17
June		Oct.			
17	17	16	15		

X Sagittar. 121.

($2^d\ 21^h$.)

Feb.		May		Aug.	
d	h	d	h	d	h
6	4	14	8	20	12
13	4	21	8	27	12
20	5	28	8	Sept.	
27	5	June		3	12
Mar.		4	9	10	13
5	5	11	9	17	13
12	5	18	9	24	13
19	6	25	10	Oct.	
26	6	July		1	14
Apr.		2	10	8	14
2	6	9	10	15	14
9	6	16	10	22	14
16	7	23	11	29	15
23	7	30	11	Nov.	
30	7	Aug.		5	15
May		6	11	12	15
7	8	13	12	19	16
				26	16

Y Ophiuchi 122.

($7^h\ 20^h$.)

Jan.		Apr.		Aug.	
d	h	d	h	d	h
14	21	26	17	7	13
Feb.		May		24	16
1	0	13	20	Sept.	
18	3	30	23	10	20
Mar.		June		27	23
6	7	17	3	Oct.	
23	10	July		15	2
Apr.		4	6	Nov.	
9	13	21	10	1	6

W Sagittar. 123.

($3^d\ 0^h$.)

Feb.		May		Aug.	
d	h	d	h	d	h
13	6	14	9	21	3
20	20	21	23	28	17
28	10	29	14	Sept.	
Mar.		June		5	7
7	1	6	4	12	21
14	15	13	18	20	12
22	5	21	9	28	2
29	20	28	23	Oct.	
Apr.		July		5	16
6	10	6	13	13	7
14	0	14	3	20	21
21	14	21	18	28	11
29	5	29	8	Nov.	
May		Aug.		5	1
6	19	5	22	12	16
		13	12	20	6
				27	20

Y Sagittar. 125.

($1^d\ 19^h$.)

Feb.		May		Aug.	
d	h	d	h	d	h
5	23	14	1	20	3
11	18	19	20	25	21
17	12	25	14	31	16
25	7	31	9	Sept.	
29	1	June		6	10
Mar.		6	3	12	5
5	20	11	21	17	23
11	14	17	16	23	18
17	9	23	10	29	12
23	3	29	5	Oct.	
28	2	July		11	11
Apr.		4	23	7	20
3	16	10	18	16	20
9	10	16	12	22	14
15	5	22	7	28	8
20	23	28	1	Nov.	
26	18	Aug.		3	3
May		2	20	8	21
2	12	8	14	14	16
8	7	14	8	20	10
				26	5

U Sagittar. 127.

($2^d\ 23^h$.)

Feb.		Apr.		June	
d	h	d	h	d	h
4	14	5	7	5	0
11	8	12	1	11	18
18	2	18	19	18	12
24	20	25	13	25	6
Mar.		May		July	
2	14	2	7	1	23
9	7	9	0	8	17
16	1	15	18	15	11
22	19	22	12	22	5
29	13	29	6	28	23

U Sagittar. 127

(*continued*).

Aug.		Sept.		Oct.	
d	h	d	h	d	h
4	17	14	4	24	15
11	11	20	22	31	9
18	5	27	16	Nov.	
24	22	Oct.		7	3
31	16	4	10	13	21
Sept.		11	4	20	15
7	10	17	21	27	9

β Lyræ 130.

(Max. $3^d\ 5^h$.
Secondary Min.
$6^d\ 11^h$.
Max. $9^d\ 16^h$.)

Jan.		May		Sept.	
d	h	d	h	d	h
11	7	6	13	12	17
24	5	19	11	25	15
Feb.		June		Oct.	
6	3	1	9	8	13
19	1	14	7	21	11
Mar.		27	5	Nov.	
2	23	July		3	9
15	21	10	3	16	7
28	19	23	1	29	5
Apr.		Aug.		Dec.	
10	17	4	23	12	2
23	15	17	21	25	0
		30	19		

VARIABLES OF SHORT PERIOD (*continued*).

U AQUILÆ 136.

(2ᵈ 12ʰ.)

Month	d	h
Feb.	14	18
	21	19
	28	20
Mar.	6	21
	13	22
	20	23
	27	23
Apr.	4	0
	11	1
	18	2
	25	2
May	2	3
	9	4
	16	5
	23	6
	30	6
June	6	7
	13	8
	20	9
	27	10
July	4	10
	11	11
	18	12
	25	13
Aug.	1	14
	8	14
	15	15
	22	16
	29	17
Sept.	5	18
	12	18
	19	19
	26	20
Oct.	3	21
	10	21
	17	22
	24	23
Nov.	1	0
	8	1
	15	1
	22	2
	29	3
Dec.	6	4
	13	5

η AQUILÆ 140.

(2ᵈ 9ʰ.)

Month	d	h
Feb.	4	20
	12	0
Feb.	19	5
	26	9
Mar.	4	13
	11	17

η AQUILÆ 140 (*continued*).

Month	d	h
Mar.	18	22
	26	2
Apr.	2	6
	9	10
	16	15
	23	19
	30	23
May	8	3
	15	7
	22	12
	29	16
June	5	20
	13	0
June	20	5
	27	9
July	4	13
	11	17
	18	22
	26	2
Aug.	2	6
	9	10
	16	15
	23	19
	30	23
Sept.	7	3
	14	7
Sept.	21	12
	28	16
Oct.	4	20
	13	0
	20	5
	27	9
Nov.	3	13
	10	17
	17	22
	25	2
Dec.	2	6
	9	10
	16	14
	23	19
	30	23

S SAGITTÆ 141.

(3ᵈ 0ʰ.)

Month	d	h
Feb.	11	17
	20	2
	28	12
Mar.	7	21
	16	6
	24	15
Apr.	2	0
	10	10
	18	19
	27	4

S SAGITTÆ 141 (*continued*).

Month	d	h
May	5	13
	13	22
	22	7
	30	17
June	8	2
	16	11
	24	20
July	3	5
	11	15
	20	0
July	28	9
Aug.	5	18
	14	3
	22	12
	30	22
Sept.	8	7
	16	16
	25	1
Oct.	3	10
Oct.	11	20
	20	5
	28	24
Nov.	5	23
	14	8
	22	17
Dec.	1	3
	9	12
	17	21
	23	6

X CYGNI 151.

(6ᵈ 21½ʰ.)

Month	d	h
Jan.	3	9
	19	8
Feb.	5	3
	21	12
Mar.	8	22
	25	7
Apr.	10	16
	27	1
May	13	11
	29	20
June	15	5
July	1	14
	18	0
Aug.	3	9
	19	18
Sept.	5	4
	21	13
Oct.	7	22
	24	7
Nov.	9	17
	26	2
Dec.	12	11
	28	20

T VULPEC. 155.

(1ᵈ 1½ʰ.)

Month	d	h
Jan.	3	17
	8	3
	12	14
	17	0
	21	11
	25	21
	30	8
Feb.	3	18
	8	5
	12	15
	17	2
	21	12
	25	23
Mar.	1	9
	5	20
	10	6
	14	17
	19	3
	23	14
	28	0
Apr.	1	10
	5	21
	10	7
	14	18
	19	4
	23	15
	28	1
May	2	12
	6	22
	11	9
	15	19
	20	6
	24	16
	29	3
June	2	3
	7	0
	11	10
	15	21
	20	7
	24	18
	29	4
July	3	15
	8	1
	12	12
	16	22
	21	9
	25	19
	30	6
Aug.	3	16
	8	3
	12	13
	16	23
	21	10
	25	20
	30	7
Sept.	3	17
	8	4
	12	14
	17	1
	21	11
	25	22
	30	8
Oct.	4	19
	9	5
	13	16
	18	2
	22	13
	26	23
	31	10
Nov.	4	20
	9	7
	13	17
	18	4
	22	14
	27	1
Dec.	1	11
	5	22
	10	8
	14	19
	19	5
	23	15
	28	2

δ CEPHEI 167.

(1ᵈ 15ʰ.)

Month	d	h
Jan.	4	17
	10	2
	15	11
	20	19
	26	4
	31	13
Feb.	5	22
	11	7
	16	15
	22	0
	27	9
Mar.	3	18
	9	2
	14	11
	19	20
	25	5
	30	14
Apr.	4	22
	10	7
	15	16
	21	1
	27	10
May	1	18
	7	3
	12	2
	17	21
	23	6
	28	14
June	2	23
	8	8
	13	17
	19	2
	24	10
	29	19
July	5	4
	10	13
	15	22
	21	6
	26	15
Aug.	1	0
	6	9
	11	18
	17	2
	22	11
	27	20
Sept.	2	5
	7	14
	12	22
	18	7
	23	16
	29	1
Oct.	4	9
	9	18
	15	3
	20	12
	25	21
	31	5
Nov.	5	14
	10	23
	16	8
	21	17
	27	1
Dec.	2	10
	7	19
	13	4
	18	13
	23	21
	29	6

MAGNETIC ELEMENTS, ROYAL OBSERVATORY, GREENWICH.

Year.	Declination.	Horizontal force.	Dip.
	° ′		° ′
1880, observed	18 33 west.	0·1804	67 36
1890, „	17 29 „	0·1823	67 23
1892, inferred	17 20 „	0·1825	67 21

The horizontal force is given in C. G. S. measure.

MINIMA OF VARIABLE STARS OF THE ALGOL TYPE.

(The figures following star's name are the Nos. in Catalogue, pp. 23–24.)

U CEPHEI 5 *.

($2^d\ 11^h\ 50^m$.)

Month	d	h	m
Jan.	2	20	6
	7	19	46
	12	19	26
	17	19	5
	22	18	45
	27	18	25
Feb.	1	18	5
	6	17	44
	11	17	24
	16	17	4
	21	16	43
	26	16	23
March.	2	16	3
	7	15	43
	12	15	22
	17	15	2
	22	14	42
	27	14	22
April.	1	14	1
	6	13	41
	11	13	21
	16	13	1
	21	12	40
	26	12	20
May.	1	12	0
	6	11	39
	11	11	19
	16	10	59
	21	10	39
	26	10	18
	31	9	58
June.	5	9	38
	10	9	18
	15	8	57
	20	8	37
	25	8	17
	30	7	57
July.	5	7	36
	10	7	16
	15	6	56
	20	6	36
	25	6	15
	30	5	55
August.	4	5	35
	9	5	14
	14	4	54
	19	4	34
	24	4	14
	29	3	53
Sept.	3	3	33
	8	3	13
	13	2	53
	18	2	32
	23	2	12
	28	1	52
Oct.	3	1	32
	8	1	11
	13	0	51
	18	0	31
	23	0	11
	27	23	50
Nov.	1	23	30
	6	23	10
	11	22	50
	16	22	29
	21	22	9
	26	21	49
Dec.	1	21	19
	6	21	8
	11	20	48
	16	20	28
	21	20	8
	26	19	47
	31	19	27

ALGOL 18 *.

($2^d\ 20^h\ 49^m$.)

Month	d	h	m
Jan.	1	16	44
	7	10	22
	13	3	59
	18	21	37
	24	15	15
	30	8	52
Feb.	5	2	30
	10	20	8
	16	13	46
	22	7	23
	28	1	1
March.	4	18	39
	10	12	16
	16	5	54
	21	23	32
	27	17	10
April.	2	10	47
	8	4	25
	13	22	3
	19	15	40
	25	9	18
June.	24	14	24
	30	8	2
July.	6	1	39
	11	19	17
	17	12	55
	23	6	33
	29	0	10
August.	3	17	48
	9	11	26
	15	5	3
	20	22	41
	26	16	19
Sept.	1	9	57
	7	3	34
	12	21	12
	18	14	50
	24	8	27
	30	2	5
Oct.	5	19	43
	11	13	20
	17	6	58
	23	0	36
	28	18	14
Nov.	3	11	51
	9	5	29
	14	23	7
	20	16	44
	26	10	22
Dec.	2	4	0
	7	21	38
	13	15	15
	19	8	53
	25	2	31
	30	20	8

λ TAURI 20 *.

($3^d\ 22^h\ 52^m$.)

Month	d	h	m
Jan.	4	18	6
	12	15	50
	20	13	35
	28	11	19
Feb.	5	9	3
	13	6	48
	21	4	32
	29	2	17
March.	8	0	1
	15	21	45
	23	19	30
	31	17	14
July.	4	14	7
	12	11	51
	20	9	36
	28	7	20

λ TAURI 20 *

(*continued*).

Month	d	h	m
August.	5	5	5
	13	2	49
	21	0	33
	28	22	18
Sept.	5	20	2
	13	17	47
	21	15	31
	29	13	15
Oct.	7	11	0
	15	8	44
	23	6	39
	31	4	13
Nov.	8	1	0
	15	23	45
	23	21	29
Dec.	1	19	14
	9	16	40
	17	14	17
	25	12	12

R CANIS MAJ. 39 †.

$\left\{\begin{matrix} 1 = 1^d\ 3^h\ 16^m. \\ 2 = 2\ \ 6\ \ 31 \end{matrix}\right\}$

Month	d	h	m
Jan.	1	1	18
	4	11	5
	7	20	52
	11	6	40
	14	16	27
	18	2	14
	21	12	1
	24	21	49
	28	7	36
	31	17	23
Feb.	4	3	11
	7	12	58
	10	22	45
	14	8	33
	17	18	20
	21	4	7
	24	13	54
	27	23	42
March.	2	9	29
	5	19	16
	9	5	4
	12	14	51
	16	0	38
	19	10	26
	22	20	13
	26	6	0
	29	15	47
April.	2	1	35
	5	11	22
	8	21	9
	12	6	57
	15	16	44
	19	2	31
	22	12	15
	25	22	6
	29	7	53
May.	2	17	40
	6	3	28
	9	13	15
	12	23	2
	16	8	50
	19	18	37
	23	4	24
	26	14	12

* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

Minima of Variable Stars of the Algol Type (continued).

R Canis Maj. 39†

(*continued*).

Sept.

d	h	m
1	6	47
4	16	35
8	2	22
11	12	9
14	21	57
18	7	44
21	17	31
25	3	19
28	13	6
Oct.		
1	22	53
5	8	40
8	18	28
12	4	15
15	14	2
18	23	50
22	9	37
25	19	24
29	5	12

Nov.

d	h	m
1	14	59
5	0	46
8	10	33
11	20	21
15	6	8
18	15	55
22	1	43
25	11	30
28	21	17
Dec.		
2	7	5
5	16	52
9	2	39
12	12	26
15	22	14
19	8	1
22	17	48
26	3	36
29	13	23

S Cancri 52.

Jan.

d	h	m
9	2	22
18	13	59
28	1	37
Feb.		
6	13	15
16	0	53
25	12	30
March.		
6	0	8
15	11	46
24	23	24
April.		
3	11	1
12	22	39
22	10	17
May.		
1	21	55
11	9	32
20	21	10
30	8	48

June.

d	h	m
8	20	26
18	8	3
28	19	41
July.		
7	7	19
16	18	57
26	6	34
Sept.		
2	5	5
11	16	43
21	4	21
30	15	59
Oct.		
10	3	36
19	15	14
29	2	52
Nov.		
7	14	30
17	2	7
26	13	45
Dec.		
6	1	23
15	13	1
25	0	38

δ Libræ 89 *.

($2^{d}\ 7^{h}\ 51^{m}$.)

Jan.

d	h	m
2	0	5
6	15	48
11	7	31
15	23	14
20	14	56
25	6	39
29	22	22
Feb.		
3	14	5
8	5	47
12	21	30
17	13	13
22	4	56
26	20	38
March.		
2	12	21
7	4	4
11	19	47
16	11	29
21	3	12
25	18	55
30	10	38
April.		
4	2	21
8	18	3
13	9	46
18	1	29
22	17	12
27	8	54
May.		
2	0	37
6	16	20
11	8	3
15	23	45
20	15	28
25	7	11
29	22	54

June.

d	h	m
3	14	36
8	6	19
12	22	2
17	13	45
22	5	27
26	21	10
July.		
1	12	53
6	4	36
10	20	18
15	12	1
20	3	44
24	19	27
29	11	10
August.		
3	2	52
7	18	35
12	10	18
17	2	1
21	17	43
26	9	26
31	1	9
Sept.		
4	16	52
9	8	34
14	0	17
18	16	0
23	7	43
27	23	25
Dec.		
2	3	24
6	19	7
11	10	50
16	2	32
20	18	15
25	9	58
30	1	41

U Coronæ 91 *.

($3^{d}\ 10^{h}\ 51^{m}$.)

Jan.

d	h	m
3	6	29
10	4	11
17	1	53
23	23	35
30	21	17

Feb.

d	h	m
6	18	59
13	16	42
20	14	24
27	12	6

U Coronæ 91*

(*continued*).

March.

d	h	m
5	9	48
12	7	30
19	5	12
26	2	54
April.		
2	0	37
8	22	19
15	20	1
22	17	43
29	15	25
May.		
6	13	7
13	10	49
20	8	31
27	6	14
June.		
3	3	56
10	1	38
16	23	20
23	21	2
30	18	44
July.		
7	16	26
14	14	8
21	11	51
28	9	33

August.

d	h	m
4	7	15
11	4	57
18	2	39
25	0	21
31	22	3
Sept.		
7	19	46
14	17	28
21	15	10
28	12	52
Oct.		
5	10	34
12	8	16
19	5	58
26	3	40
29	14	31
Nov.		
5	12	14
12	9	56
19	7	38
26	5	20
Dec.		
3	3	2
10	0	44
16	22	26
23	20	8
30	17	51

U Ophiuchi 120‡.

$$\left\{\begin{array}{llll} 1 = 0^{d} & 20^{h} & 8^{m} \\ 2 = 1 & 16 & 16 \\ 3 = 2 & 12 & 22 \end{array}\right\}$$

Jan.

d	h	m
1	18	37
5	3	7
8	11	38
11	20	9
15	4	39
18	13	10
21	21	41
25	6	11
28	14	42
31	23	13
Feb.		
4	7	43
7	16	14
11	0	45
14	9	15

Feb.

d	h	m
17	17	46
21	2	17
24	10	47
27	19	18
March.		
2	3	49
5	12	19
8	20	50
12	5	21
15	13	51
18	22	22
22	6	52
25	15	23
28	23	54

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* Alternate minima only are given; the others can be readily found by adding the interval under the name of the star.

† Every third minimum only.

‡ Every fourth minimum only.

Minima of Variable Stars of the Algol Type (continued).

U Ophiuchi 120 ‡ (*continued*).

April.

d	h	m
1	8	24
4	16	55
8	1	26
11	9	56
14	18	27
18	2	58
21	11	28
24	19	59
28	4	30

May.

d	h	m
1	13	0
4	21	31
8	6	2
11	14	32
14	23	3
18	7	33
21	16	4
25	0	35
28	9	5
31	17	36

June.

d	h	m
4	2	7
7	10	37
10	19	8
14	3	29
17	12	9
20	20	40
24	5	11
27	13	41
30	22	12

July.

d	h	m
4	6	43
7	15	13
10	23	44
14	8	14
17	16	45
21	1	16
24	9	46
27	18	17
31	2	48

August.

d	h	m
3	11	18
6	19	49
10	4	20
13	12	50
16	21	21
20	5	52
23	14	22
26	22	53
30	7	24

Sept.

d	h	m
2	15	54
6	0	25
9	8	55
12	17	26
16	1	57
19	10	27
22	18	58
26	3	29
29	11	59

Oct.

d	h	m
2	20	30
6	5	1
9	13	31
12	22	2
16	6	33
19	15	3
22	23	34
26	8	4
29	16	35

Y Cygni 156 ‡.

$$\left\{\begin{array}{l} 1=1^{d}\ 11^{h}\ 56^{m}\\ 2=2\ \ 23\ \ 53\\ 3=4\ \ 11\ \ 49\end{array}\right\}$$

Jan.

d	h	m
1	4	17
7	4	2
13	3	47
19	3	32
25	3	16
31	3	1

Feb.

d	h	m
6	2	45
12	2	30
18	2	15
24	1	59

Y Cygni 156 ‡ (*continued*).

March.

d	h	m
1	1	44
7	1	29
13	1	13
19	0	58
25	0	43
31	0	28

April.

d	h	m
6	0	13
11	23	58
17	23	43
23	23	28
29	23	14

May.

d	h	m
5	22	59
11	22	45
17	22	30
23	22	16
29	22	2

June.

d	h	m
4	21	48
10	21	34
16	21	21
22	21	7
28	20	54

July.

d	h	m
4	20	40
10	20	27
16	20	14
22	20	2
28	19	49

August.

d	h	m
3	19	37
9	19	25
15	19	12
21	19	1
27	18	49

Sept.

d	h	m
2	18	37
8	18	26
14	18	15
20	18	4
26	17	53

Oct.

d	h	m
2	17	42
8	17	32
14	17	22
20	17	12
26	17	2

Nov.

d	h	m
1	16	52
7	16	43
13	16	33
19	16	24
25	16	15

Dec.

d	h	m
1	16	7
7	15	58
13	15	49
19	15	41
25	15	33
31	15	25

S Antliæ 56 §.

Jan.

d	h	m
1	3	43
4	9	31
7	15	19
10	21	7
14	2	55
17	8	43
20	14	31
23	20	19
27	2	7
30	7	55

Feb.

d	h	m
2	13	43
5	19	31
9	1	19
12	7	7
15	12	55
18	18	43
22	0	31
25	6	19
28	12	7

S Antliæ 56 § (*continued*).

March.

d	h	m
2	17	55
5	23	43
9	5	31
12	11	19
15	17	7
18	22	55
22	4	43
25	10	31
28	16	19
31	22	7

April.

d	h	m
4	3	55
7	9	43
10	15	31
13	21	19
17	3	7
20	8	55
23	14	43
26	20	31
30	2	19

May.

d	h	m
3	8	7
6	13	55
9	19	43
13	1	31
16	7	19
19	13	7
22	18	55
26	0	43
29	6	31

June.

d	h	m
1	12	19
4	18	7
7	23	55
11	5	43
14	11	31
17	17	19
20	23	7
24	4	55
27	10	43
30	16	31

July.

d	h	m
3	22	19
7	4	7
10	9	55
13	15	43
16	21	31
20	3	19

Sept.

d	h	m
3	12	31
6	18	19
10	0	7
13	5	55
16	11	43
19	17	31
22	23	19
26	5	7
29	10	55

Oct.

d	h	m
2	16	43
5	22	31
9	4	19
12	10	7
15	15	55
18	21	43
22	3	31
25	9	19
28	15	7
31	20	55

Nov.

d	h	m
4	2	43
7	8	31
10	14	19
13	20	7
17	1	55
20	7	43
23	13	31
26	19	19
30	1	7

Dec.

d	h	m
3	6	55
6	12	43
9	18	31
13	0	19
16	6	7
19	11	55
22	17	43
25	23	31
29	5	19

‡ Every fourth minimum only.

§ Every tenth minimum only, the others may be found by adding the multiples of period in the following table:—

P.	h	m	P.	d	h	m	P.	d	h	m
	7	47	4	1	7	7	7	2	6	28
2	15	34	5	1	14	54	8	2	14	14
3	23	20	6	1	22	41	9	2	22	1

MAXIMA AND MINIMA OF VARIABLE STARS.

The No. refers to catalogue on pp. 23–24; M, signifies maximum; *m*, minimum.

January.

d	No.	
2	78	*m*
	157	M
	75	M
3	138	M
4	170	M
10	163	*m*
11	30	M
12	66	M
	131	*m*
	37	*m*
14	146	*m*
	60	M
15	73	*m*
16	16	M
19	83	*m*
	11	M
	23	M
22	114	M
	41	*m*
25	60	*m*
26	98	M
	67	M
27	131	M
28	57	M
30	169	M
31	147	M
	108	M

February.

d	No.	
1	92	*m*
2	117	M
	137	M
3	129	M
	115	M
4	8	*m*
	64	M
6	47	M
	105	M
7	62	*m*
8	63	M
9	41	M
11	124	M
13	107	M
14	60	M
15	87	M
16	88	M
18	146	*m*
19	138	*m*
20	15	*m*
24	174	M
25	21	M
	60	*m*
27	131	*m*
28	158	M

March.

d	No.	
1	38	*m*
2	71	*m*
3	144	*m*
5	109	M
	132	*m*
6	74	M
8	41	*m*
9	72	*m*
10	25	M
	129	*m*
	138	M
11	40	M
12	163	M
13	131	M
	79	M
	50	M
	27	*m*
14	168	M
	157	*m*
	36	*m*
15	9	M
16	60	M
17	44	M
18	112	M
20	48	M
	153	M
	31	*m*
21	54	M
23	143	M
24	73	M
	146	*m*
25	32	M
	139	*m*
26	41	M
	116	*m*
27	60	*m*
28	101	M
	12	M
29	80	M
30	94	M
31	59	*m*

April.

d	No.	
2	61	M
3	53	M
4	154	*m*
5	19	M
7	90	M
9	66	*m*
11	29	*m*
12	83	M
13	131	*m*
	111	M
14	126	M
14	129	M
	102	M
	93	M
16	60	M
17	13	M
19	97	M
	148	*m*
	119	M
22	41	*m*
25	150	*m*
26	46	M
27	118	M
	138	*m*
	42	M
	60	*m*
28	131	M
	146	*m*
29	95	M

May.

d	No.	
1	75	*m*
2	47	M
	15	M
3	166	M
4	8	M
	10	M
5	62	M
	37	M
6	106	M
7	124	*m*
9	88	*m*
10	41	M
	172	M
11	38	M
	92	M
	173	M
14	84	M
15	163	*m*
	17	*m*
17	43	M
	60	M
	128	M
	85	*m*
	138	M
18	116	M
19	55	*m*
	157	M
20	129	*m*
21	1	*m*
	83	*m*
23	34	M
26	3	M
28	60	*m*
	134	M
29	131	*m*
30	103	M
31	77	M

June.

d	No.	
2	71	M
3	146	*m*
	45	M
6	41	*m*
7	81	M
8	73	*m*
12	162	M
13	131	M
16	87	*m*
17	60	M
19	117	*m*
20	66	M
22	9	*m*
	12	*m*
23	57	*m*
24	41	M
	129	M
27	154	M
28	60	*m*
30	72	M
	36	M

July.

d	No.	
2	96	M
	74	*m*
4	138	*m*
	115	*m*
6	144	*m*
7	145	M
	146	*m*
8	104	M
12	160	M
	28	*m*
13	86	M
14	131	*m*
15	38	*m*
16	163	M
17	165	M
18	60	M
	78	M
20	24	M
	132	M
21	41	*m*
22	135	M
24	138	M
	124	M
	99	M
26	108	M
26	144	*m*
27	51	M
	8	*m*
	47	M
28	75	M
29	40	*m*
	60	*m*
	131	M
30	157	*m*
	129	*m*

August.

d	No.	
1	22	M
3	158	M
5	15	*m*
6	67	M
7	150	M
8	4	M
	41	M
	88	M
10	65	M
11	92	*m*
12	146	*m*
	26	M
13	83	M
16	73	M
	68	M
18	60	M
20	62	*m*
	164	*m*
23	63	*m*
	105	M
26	59	M
	25	M
	21	*m*
29	60	*m*
	131	*m*
	50	*m*

September.

d	No.	
2	159	M
3	129	M
4	41	*m*
6	161	M
	58	M
7	16	M
9	138	*m*
11	7	M
12	114	M
	139	M
	84	*m*
	85	M
13	131	M
15	17	M
16	146	*m*
	66	*m*
18	163	*m*
	60	M
21	83	*m*
22	63	*m*
	41	M
23	69	M
24	38	M
	107	M
26	87	M
28	71	*m*
	171	M
29	60	*m*
30	138	M
	44	*m*

October.

d	No.	
1	12	M
4	118	*m*
	157	M
5	117	M
	70	M
8	42	*m*
	9	M
9	129	*m*
10	153	M
14	131	*m*
16	15	M
18	74	M
19	124	*m*
	60	M
20	41	*m*
21	146	*m*
	47	M
25	154	*m*
	8	M
26	110	M
28	29	M
	82	M
29	131	M
30	60	*m*
	88	*m*
31	27	M

November.

d	No.	
1	73	*m*
	19	M
4	169	M
	11	M
5	31	*m*
7	41	M
10	147	M
	14	M
10	49	M
12	142	M
13	129	M
14	116	M
	152	M
15	62	M
16	98	M
	138	*m*
17	113	M
18	115	M
19	163	M
	60	M
	92	M
21	72	*m*
22	13	*m*
24	75	*m*
	174	*m*
25	94	*m*
	146	*m*
27	66	M
28	38	*m*
29	131	*m*
30	60	*m*

December.

d	No.	
4	41	*m*
5	97	M
	111	*m*
	148	M
6	57	M
7	138	M
8	63	M
9	50	M
10	2	M
12	40	M
14	83	M
	131	M
15	157	*m*
	37	*m*
16	53	M
	106	M
17	79	*m*
19	129	*m*
20	60	M
	144	*m*
21	100	M
22	41	M
	133	M
26	12	*m*
29	71	M
30	34	*m*
	149	M
31	146	*m*
	60	*m*

DOUBLE STARS.

No.	Name of Star.	R.A. 1890.	Dec. 1890.	Mag.	Position.	Distance.	Epoch. 1880+.	Observer.
		h m	° ′		°	″		
1.	Σ 13, Cephei 318	0 10·0	+76 20	7 ; 7	91·7	0·81	9·41	β.
2.	OΣ 4	0 10·5	+35 52	7 ; 8	141·1	0·30	7·93	HΣ.
3.	OΣ 12, λ Cassiop.	0 25·7	+53 55	6½ ; 6½	144·2	0·45	7·52	T.
4.	Σ 60, η Cassiop.	0 42·5	+57 14	3½ ; 7½	191·8	4·79	11·75	M.
5.	OΣ 20, 66 Piscium ..	0 48·8	+18 36	6 ; 7	350·7	0·42	8·78	HΣ.
6.	Σ 73, 36 Andromedæ .	0 48·9	+23 2	6 ; 6½	9·2	1·35	11·08	M.
7.	Σ 113, 42 Ceti	1 14·0	− 1 5	6½ ; 7	349·6	1·51	8·44	L.
8.	Σ 138	1 30·3	+ 7 5	7 ; 7	216·1	1·53	11·08	M.
9.	*p* Eridani	1 35·7	−56 45	6 ; 6	227·2	7·02	10·01	Pk.
10.	Σ 186	1 51·0	+ 1 20	7 ; 7	227·1	0·31	10·88	β.
11.	Σ 208, 10 Arietis	1 57·4	+25 24	6 ; 8	49·2	0·93	8·05	S.
12.	Σ 228	2 6·9	+46 58	6½ ; 7½	30·8	0·35	9·02	S.
13.	Hastings	2 11·0	−18 44	8 ; 9	341·4	2·21	10·95	β.
14.	Σ 333, ε Arietis.........	2 52·9	+20 54	5½ ; 6	201·4	1·32	10·99	M.
15.	Σ 367	3 8·0	+ 0 20	7½ ; 7½	229·4	0·84	8·06	L.
16.	Σ 412, 7 Tauri	3 27·9	+24 6	6½ ; 7	216·4	0·30	9·64	β.
17.	Σ 422........................	3 32·5	+ 0 16	6 ; 8	244·4	6·20	8·94	T.
18.	OΣ 79, 55 Tauri	4 13·6	+16 16	6½ ; 8	87·9	0·38	10·67	β.
19.	Σ 535	4 17·2	+11 7	7 ; 8	333·8	1·38	8·47	L.
20.	OΣ 98, ι Orionis	5 1·9	+ 8 21	5½ ; 7	193·9	0·99	9·03	L.
21.	Σ 728	5 24·9	+ 5 52	6 ; 8	179·2	0·38	9·03	β.
22.	Σ 749	5 30·3	+26 51	6½ ; 7	174·1	1·02	7·18	T.
23.	Σ 948, 12 Lyncis AB .	6 36·5	+59 33	6 ; 6½	124·9	1·57	9·31	M.
24.	Sirius.........................	6 40·3	−16 34	1 ; 10	359·7	4·19	10·27	β.
25.	OΣ 159, 15 Lyncis ...	6 47·8	+58 34	5 ; 6	5·9	0·77	10·25	M.
26.	OΣ 170	7 12·1	+ 9 30	7 ; 7½	110·9	1·65	11·22	M.
27.	Σ 1110, Castor AB ...	7 27·0	+32 7	2 ; 3	230·4	5·66	9·25	M.
28.	β 101, 9 Argûs	7 46·7	−13 35	5 ; 7	88·3	0·36	10·96	β.
29.	OΣ 182	7 47·2	+ 3 40	8½ ; 8½	34·2	1·15	8·12	L.
30.	Σ 1157	7 49·0	− 2 31	8 ; 8	247·0	1·28	7·60	T.
31.	Σ 1196, ζ Cancri AB .	8 5·3	+18 0	5 ; 6	33·7	1·22	11·26	M.
32.	β 205	8 28·0	−24 14	7 ; 7	81·9	0·70	10·28	β.
33.	Σ 1273, ε Hydræ	8 41·0	+ 6 49	4 ; 7	227·6	3·16	10·35	M.
34.	Σ 3121	9 11·3	+29 3	7½ ; 7½	286·3	0·22	8·27	S.
35.	Σ 1356, ω Leonis	9 22·6	+ 9 32	5½ ; 7	101·8	0·68	10·27	C.
36.	Σ 1417	10 9·0	+19 42	8½ ; 8½	258·3	2·41	8·83	L.
37.	OΣ 215, Piazzi x. 23...	10 10·3	+18 17	6½ ; 7	213·6	0·74	10·27	C.
38.	Σ 1424, γ Leonis AB .	10 13·9	+20 24	2 ; 4	114·5	3·36	10·27	C.
39.	Σ 1457	10 32·0	+ 6 20	7½ ; 8	313·5	1·21	9·70	M.
40.	OΣ 224	10 34·1	+ 9 26	7 ; 9	315·6	0·53	7·27	S.
41.	Σ 1523, ξ Ursæ Maj. .	11 12·3	+32 9	4 ; 5	199·9	1·74	11·47	M.
42.	Σ 1536, ι Leonis	11 18·2	+11 8	4½ ; 7½	62·0	2·56	9·16	L.
43.	OΣ 234	11 24·8	+41 54	7 ; 7½	324	0·20	6·36	E.
44.	OΣ 235	11 26·1	+61 42	5½ ; 7	86·4	1·04	11·54	M.
45.	OΣ 287	11 47·6	+45 22	7 ; 8	317·5	0·64	7·56	S.
46.	Σ 1639, 68 Comæ......	12 19·0	+26 12	6½ ; 8	236·0	0·22	7·39	S.
47.	Σ 1670, γ Virginis ...	12 36·1	− 0 51	3 ; 3	153·8	5·48	11·46	M.
48.	Σ 1687, 35 Comæ......	12 47·9	+21 51	5 ; 8	69·5	1·30	7·34	T.
49.	Σ 1728, 42 Comæ......	13 4·7	+18 7	5½ ; 6	9·3	0·70	10·33	β.
50.	Σ 1757, Piazzi xiii. 127	13 28·9	+ 0 15	7½ ; 8	72·6	2·50	10·06	M.
51.	Σ 1768, 25 Can. Ven. .	13 33·0	+36 46	6 ; 8	143·6	0·93	11·51	M.
52.	Σ 1788	13 49·2	− 7 31	6½ ; 7½	76·1	2·60	11·12	M.
53.	Σ 1819	14 9·8	+ 3 39	7½ ; 7½	189·5	1·42	11·49	M.
54.	α Centauri..................	14 32·0	−60 33	1 ; 2	204·1	18·04	9·53	Pk.

DOUBLE STARS (*continued*).

No.	Name of Star.	R.A. 1890.	Dec. 1890.	Mag.	Position.	Distance.	Epoch 1880+.	Observer.
		h m	° ′		°	″		
55.	Σ 1865, ζ Boötis	14 36·2	+14 12	4 ; 4	293·4	0·51	7·50	S.
56.	Σ 1876	14 40·8	− 6 55	8 ; 8	251·5	1·30	8·31	L.
57.	Σ 1888, ξ Boötis	14 46·3	+19 33	5 ; 7	243·4	3·18	11·48	M.
58.	β 239	14 52·0	−27 12	6 ; 6	311·4	0·86	9·44	β.
59.	β 119	14 59·7	− 6 35	8 ; 8½	306·4	1·61	8·45	L.
60.	Σ 1937, η Cor. Bor.	15 18·8	+30 42	6 ; 6	222·0	0·75	11·54	M.
61.	Σ 1938, μ^2 Boötis	15 20·4	+37 46	6½ ; 7½	97·8	0·73	9·35	M.
62.	OΣ 298	15 32·5	+40 13	7 ; 7½	143·0	0·33	7·56	S.
63.	Σ 1967, γ Cor. Bor.	15 35·0	+37 0	4 ; 7	126·6	0·38	7·51	S.
64.	OΣ 303	15 56·0	+13 35	7 ; 7½	139·9	0·97	11·52	M.
65.	Σ 1998, ξ Libræ	15 57·7	−11 4	5 ; 5½	200·4	1·24	8·50	L.
66.	Σ 2032, σ Coronæ	16 11·0	+34 12	6 ; 6½	207·8	4·08	10·33	β.
67.	Σ 2055, λ Ophiuchi	16 25·0	+ 2 13	4½ ; 5½	44·2	1·65	11·64	M.
68.	Σ 2084, ζ Herculis	16 37·2	+31 48	3 ; 6½	60·1	1·40	11·63	M.
69.	Σ 2107, 167 Herculis	16 47·6	+28 52	7 ; 8½	244·6	0·49	7·51	S.
70.	Σ 2120, 210 Herculis	17 0·0	+28 15	6 ; 9	248·1	6·09	8·67	M.
71.	Σ 2173, 221 Ophiuchi	17 24·5	− 0 58	6 ; 6	343·1	0·84	10·69	M.
72.	β 631, 255 Ophiuchi	17 33·0	− 0 25	7 ; 7	238·8	0·40	8·51	L.
73.	OΣ 338	17 47·0	+15 21	6½ ; 6½	21·2	0·84	9·73	M.
74.	Σ 2262, τ Ophiuchi	17 57·1	− 8 11	5 ; 6	255·2	1·72	8·71	M.
75.	Σ 2272, 70 Ophiuchi	18 0·0	+ 2 33	4 ; 6	328·3	2·11	11·54	M.
76.	Σ 2315	18 21·0	+27 20	7 ; 8	226·5	0·27	7·56	S.
77.	OΣ 358	18 31·0	+16 50	6½ ; 7	198·4	1·73	8·74	M.
78.	γ Coronæ Australis	18 59·0	−37 14	5½ ; 5½	185·4	1·79	9·41	β.
79.	β 248	19 12·9	+22 51	6 ; 9	125·4	1·86	10.63	β.
80.	Schjellerup 30	19 21·5	−12 22	8 ; 8	328·3	1·60	8·54	L.
81.	OΣ 387	19 44·5	+35 2	7 ; 8	354·6	0·55	8·76	HΣ.
82.	OΣ 400	20 6·8	+43 37	7 ; 8	140·1	0·25	7·92	S.
83.	OΣ 406	20 16·4	+45 1	7 ; 8	108·9	0·49	7·51	S.
84.	β 151, β Delphini	20 32·4	+14 13	3½ ; 5½	324·2	0·45	10·49	β.
85.	OΣ 413, λ Cygni	20 43·1	+36 8	5 ; 7½	70·5	0·66	9·86	M.
86.	Σ 2729	20 46·0	− 6 2	6½ ; 6½	170·5	0·53	7·82	T.
87.	Σ 2737, AB	20 54·0	+ 3 53	6 ; 7	283·5	0·90	9·82	M.
88.	Σ 2758, 61 Cygni	21 2·0	+38 13	5 ; 6	121·2	20·58	8·84	M.
89.	OΣ 535, δ Equulei	21 9·0	+ 9 34	4½ ; 5	193·1	0·2 ±	9·82	H.
90.	β 838	21 15·3	+ 2 40	7½ ; 9½	95·8	1·71	7·77	C.
91.	Σ 2799	21 23·3	+10 36	7 ; 7½	302·9	1·35	9·94	M.
92.	β 170	22 3·0	−19 1	8 ; 8	60·4	1·56	8·78	L.
93.	Σ 2909, ζ Aquarii	22 23·2	− 0 36	4 ; 4	324·0	3·10	11·78	M.
94.	OΣ 489, π Cephei	23 4·4	+74 47	5 ; 7½	30·0	1·05	9·73	L.
95.	β 80	23 13·0	+ 4 48	8 ; 9	319·5	0·84	8·79	C.
96.	β 854	23 19·0	+ 5 26	8½ ; 8½	88·3	1·89	8·82	L.
97.	Σ 3046	23 51·0	−10 7	8 ; 8	246·9	2·98	8·50	L.
98.	β 733	23 56·7	+26 30	6 ; 9	139·0	0·78	10·55	β.
99.	β 281	23 57·1	+ 1 31	8 ; 9	209·0	1·35	8·89	L.

In the column headed "Observer," the following abbreviations are used:—

L.	Leavenworth.	β.	Burnham.	H.	Hough.	Pk.	Pollock.
S.	Schiaparelli.	E.	Engelmann.	T.	Tarrant.		
HΣ.	Herman Struve.	M.	Maw.	C.	Comstock.		

THE OBSERVATORY,

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MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, December 11, 1891.

E. J. Stone, M.A., F.R.S., *Vice-President* (Past President), in the Chair.

Secretaries: A. M. W. Downing, M.A., and E. W. Maunder.

The Chairman. I am sorry to say that our President is ill, and also that our senior Secretary, Mr. Knobel, is unable to be here. Mr. Maunder will therefore read the Minutes of the last Meeting.

The Minutes of the last Meeting were read and confirmed.

Mr. Maunder. Sixty-five presents have been received since the last Meeting. Amongst those calling for special mention are:—'Opera di Galileo,' vol. ii., presented by the Italian Government; Vienna Observatory 'Annalen,' Band vii., presented by the Observatory; Munich Observatory 'Neue Annalen,' Band ii., presented by the Observatory; Paris Observatory 'Catalogue,' tome ii. (6^h to 12^h), presented by the Observatory; 'Philosophical Transactions of the Royal Society,' vol. i. (containing Ball's drawings of Saturn), presented by Mr. Prince; Two original negatives of the Moon, presented by the Lick Observatory; 'The Mariner's Magazine' and Leybourn's 'Art of Dialling,' presented by Mr. Schooling.

A vote of thanks was accorded to the donors of the presents.

Mr. Ranyard. I believe this is the night on which Auditors should be appointed. I beg to propose that Mr. W. H. Maw, Mr. R. Inwards, and Mr. R. J. Lecky be appointed Auditors of the Treasurer's accounts for 1891.

Mr. Schooling. I beg to second that.

The resolution was carried unanimously.

Mr. Downing read a paper "On the Stonyhurst Drawings of Sun-Spots and Faculæ," by the *Rev. W. Sidgreaves.*

The writer describes the method followed at the Stonyhurst Observatory, of sketching the fainter parts of the faculæ. This consists in relatively sweeping the image across the drawing-board, or really moving the drawing-board across the solar image. The eye easily rests upon any part of the image without following the board, and the details of the picture are brought out with remarkable clearness. By this help the observer can fill in and add to the outline-tracing of the stronger parts.

The paper deals mainly with the question of spots or faculæ. It admits the claim of spots to be in general the forerunners of faculæ, on the grounds that faculæ admittedly grow in extent after the birth of a spot, and often long outlive them. But it shows from the drawings of the last minimum period of 1889, that we cannot extend the conclusion to the absolute priority of the spots: for in that year two remarkable cases appear in which faculæ preceded the birth of the spot by a part of two days. And the significance of these two apparently isolated facts is brought out by noticing the great odds against the chance of gaining the positive evidence of priority: that the spot must come into existence within the limited part of the visible hemisphere where faculæ can be seen, and that it must be known from previous drawings that the faculæ, if present, are not débris of old disturbances.

The paper also draws attention to concomitant faculæ,—a small surrounding of bright faculæ which appears on the drawings of all the notable spots when first seen. And the writer considers this a confirmation of the probability that spots are really preceded for a short time by a bright but not extensive display of faculæ.

Mr. Maunder. It seems to me that Father Sidgreaves' communication opens up a point of great interest in Solar Physics. I have not had time since I first saw his paper to go thoroughly into the matter, but I took the case of a single year, the minimum year of 1888, and went through the Greenwich Sun-spot observations with this result, that out of 53 groups of spots 12 showed faculæ before the spot was seen. Of those 12, in 4 the spot came round the limb, and faculæ preceded the spot; consequently they were not instances of the kind which Father Sidgreaves speaks about, since they were not new disturbances, but simply disturbances newly come into view. Of the other eight cases seven were of the same kind. They were all very small short-lived groups of spots, and every one of those seven cases were revivals of old disturbances. That is to say, these seven groups occurred in parts of the Sun where small and transient outbreaks had been observed some time earlier, but where no disturbance had been seen in the interval. So, though the faculæ appeared before the spots in these cases, they afford no definite proof of what can happen in an entirely new disturbance. In the 12th case there seems some evidence for thinking that faculæ appeared before the spot; but in that case there was an interval of one day in which neither faculæ nor spots were seen. One day there was a group of faculæ, the next day no faculæ were observed, and on the third day a small spot appeared. It is curious that these intermittent disturbances, fixed as to their place and capable of breaking out at a considerable interval of time, should show themselves by such small and short-lived spots. Seven of these groups appeared in districts where other groups had been seen it may be a couple of months before. The earlier and later groups lasted only one or two days, and in the interval between, perhaps a day or two before the appearance of the second group of spots, the faculæ might be seen. So far, then, as my observations of that year go, there is little evidence to show that faculæ ever appear first in an entirely new disturbance.

A vote of thanks was passed to Father Sidgreaves for his paper.

Mr. Downing then read a paper by *Prof. H. C. Vogel*, "On the Spectrographic Method of determining the Velocity of Stars in the line of Sight."

Experiments in 1887 showed the extreme accuracy and sensitiveness of the photographic method of measuring the displacement of lines in star-spectra produced by motion in line of sight. The disturbances of the atmosphere affect the photographic method far less than they do the direct method. Two hundred negatives of 49 stars have been obtained and measured, and the results show that the exactness of measurement far surpasses the expectations based on the first plates. This great accuracy is due to the advantageous construction of the apparatus, its exact adjustment, and the peculiar methods adopted in measuring photographs. An explanation of this method may be of value and interest to many employed in similar work. The points considered in construction of the apparatus were great stability for smallest possible weight, suitable dimensions of prisms, collimator and camera objectives to preserve brightness with the greatest possible dispersion, accurate adjustment of photographic plate in the focal plane of the camera objective, exact keeping of the star on the slit of the spectrograph. The frame is of cast steel, of a form which offers the greatest possible resistance to flexure. The most suitable dimensions for collimator and camera objectives for the 12-inch refractor were found to be 408 mm. focal length and 34 mm. aperture.

Two Rutherfurd compound prisms of 35 mm. height (length of refracting edge) and 45 mm. breadth (perpendicular distance from the refracting edge of the flint-glass prism) were used. They are of the most colourless glass obtainable, and the dispersion from F to H for each amounts to about 5°. The camera is of sheet steel, the plate-holders of brass, and the focussing can be arranged with an accuracy of a fraction of a millimetre. A small telescope is connected with the apparatus, and receives the light reflected from the front side of the first prism. The slit, which is illuminated by a Geissler tube, appears as a fine line of light with the star in the centre, and the star can be kept thus by the slow motions of the telescope. No cylindrical lens is employed, but the slit is placed parallel to the line of diurnal motion, and by changing the rate of the driving-clock a slight widening of the spectrum is effected. The optic axis of the collimator is brought exactly into the optic axis of the refractor by three adjusting-screws.

The plane of the slit is adjusted to the focus of the $H\gamma$ line of the refractor, the prisms being set at minimum deviation for that line, and by means of a table of settings the position is varied according to the temperature. The $H\gamma$ line appears in the middle of each negative, and the adjustment of the focus of this in the camera is also according to temperature.

Hydrogen is usually used for the comparison spectrum, the Geissler tube being 40 cm. from the slit at right angles to the optical axis of the refractor as well as the slit, and therefore its light has to be regarded as dispersed on reaching the slit. The tubes used were very thin, and only 17 per cent. of the light is lost in transmission. No exact adjustments of the tube are necessary, and no changes of position of the instrument can introduce injurious effects.

Iron has been used for comparison with some of the plates of Sirius. Dr. Schleussner, of Frankfort, has provided the most suitable plates for the work.

The width of the slit is left unchanged at ·02 mm., and a uniform exposure of 1 hour is given, the observer varying the width of the spectrum to correspond to the brightness of the star. When used on the Sun the spectroscope visually and photographically shows nearly all the lines near $H\gamma$ that are shown on Rowland's map. The photographs, with the apparatus, do not show all the lines which can be observed direct with it. The prisms would bear greater magnifying-power on the part of the camera objective, but the refractor aperture is too small for this. The dispersion seems sufficient for one negative of α Aurigæ. Dr. Scheiner has measured 290 lines between 412·4 and 466·8 μ.

To test the correct adjustment, photographs of the spectrum of the Moon show the hydrogen lines absolutely coincident, while with Venus the calculated and observed velocities are :—

	Observed Velocity.	Calculated Velocity.	
1889. Jan. 2	−7·8	−7·4	English miles.
Feb. 10	−7·4	−7·8	,,

The measurement of the spectra is made by the aid of a microscope with power of from 7 to 35.

The table of the microscope carries a sliding apparatus moved by a micrometer-screw of 25 mm. pitch, to which the negative is firmly clamped. The errors of the screw have been determined, and from a series of measures of the solar spectrum one turn of the screw corresponds to a difference in wave-length of 324 μ. With this value the value g, velocity in miles per sec. corresponding to one revolution, can be calculated from the formula $g = \frac{\Delta\lambda}{\lambda} V$, where V is the velocity of light, and λ the wave-length of Hγ (434·07). The result is $g = 139{\cdot}13$ miles. This value, strictly speaking, holds good only for the temperature while taking the solar spectrum, and a small correction for temperature has been applied.

In the case of Classes II. and III., with many lines in the spectra, differential measurements between the Hγ line and other lines were made, and a plate of the solar spectrum is cut off lengthwise and laid over the star spectrum, so that the two appear to coincide in the microscope. The films are in contact, and the lines of the one spectrum form prolongations of those of the other.

Four settings are made on a Sun line and four on the corresponding Star line, and three lines nearest Hγ on either side are generally used. The difference of the readings in the spectra give the amount of the displacement, the Hγ line coinciding in both spectra. The differences are unequal, owing to temperature, and assuming a simple proportional change. A reduction to the dispersion of the solar negative must always be made, and this is done by the method of least squares. Distortions of the film are as much as possible eliminated by the method of measurement, and every prejudice of the observer shut out, since the amount of displacement is only obtained after computation.

With bright stars of Class I. iron can be used as well as hydrogen, but in fainter stars hydrogen only can be used. The difficulty of finding the part of maximum intensity in the star line presented itself here, and to overcome this the Hγ star line and the artificial line were covered by a comparatively broad strip, which was brought exactly over the middle of the Hγ line in the star. The readings of this and of the artificial line give the displacement. Dark strips of from ·05 to ·20 mm. have been thus used, the strip chosen being laid directly upon the star negative, and fastened by a simple mechanical contrivance, which allowed of its being moved by a fine screw. Another method of measurement adopted was to select the one of a number of lines of varying width (photographed on small plates) which most nearly resembled the artificial Hγ line on the negative, and to place this symmetrically with the artificial Hγ line on the diffused Hγ line of the star spectrum. The distance between these lines then equalled twice the displacement.

The first result of importance obtained was the proof of the influence of the Earth's motion on displacement, and a long table of measurements of *α* Aurigæ, *α* Tauri, *α* Ophiuchi, and *α* Ursæ Majoris illustrates this. A further result was the discovery of changes in the motion of Algol, due to the dark satellite. The discovery of the periodic motion of *a* Virginis followed. For true motion *a* Virginis gives velocity in line of sight—

$$\text{Velocity of system} = (56{\cdot}7)\sin\left(\frac{t-t_0}{p}\right)360^\circ \text{ in miles.}$$

$$\text{Velocity of system} = 9{\cdot}2; \qquad \text{period } p = 4{\cdot}0134 \text{ days.}$$

With β Aurigæ the periodical doubling of very fine lines is perceptible, as well as that of the stronger lines. A list of observations of *a* Lyræ and of *a* Canis Minoris illustrates the accuracy of the method of measuring by covering the Hγ by a strip. For Sirius the hydrogen lines give 7·3 and the iron lines 9·0 miles of approach towards Sun. The probable error of a single negative of Class II. is estimated at $\pm 1{\cdot}34$ miles, and for Class I. $\pm 2{\cdot}31$. Independent measurements have been made in each case by Dr. Scheiner and myself.

I intend after the definite completion of the measurements to communicate to the Society a list of observed velocities, and will finally remark that the velocities of the stars have proved to be smaller than was expected from direct observations. The mean result for 47 stars is 10·6 English miles. Six stars have velocities less than 2 and five greater than 20 miles. *a* Tauri is greatest at about 30 miles. Fifteen have positive and thirty-two negative motion.

Mr. Maunder. I think Prof. Vogel is much to be congratulated upon the skill he has shown in devising this spectrographic method of determining the velocity of stars in the line of sight, and on the success which has attended it so far. The photographic method has several advantages over eye observation. In the first place the atmosphere and disturbances do not seem to embarrass the photograph, as they certainly do the observer. There is nothing more difficult than to bisect the star line if the star line be unsteady. The star line frequently disappears, and to retain it in sight sufficiently long to bisect it is very difficult. The photographic method has the advantage that, by prolonging the exposure, it is possible to get an image with a dispersion which would be too great for direct observation with the eye. The same thing occurs which has happened in photographing the image of a star—just as the image of a star too faint to be seen with a given telescope, by sufficiently prolonging the exposure, may yet be photographed with the same telescope, so it is with the spectrum. Prof. Vogel's aperture is about the same as we use at Greenwich, but the scale of the spectrum on Prof. Vogel's photographs would appear to be nearly 3 times the scale that we have in the viewing telescope of the Greenwich spectroscope; so that his factor is only about one-third of that which I have to use. Then there is the further advantage that it is possible at one observation to make a comparison not between one line of the spectrum of the star and one line of the comparison spectrum, but between many comparison lines and many lines of the star spectrum. There are one or two points in Prof. Vogel's paper, however, which I am not quite inclined to agree with, although it may seem very presumptuous for me to say so, seeing that his results are so much more accordant than my own; but it does not seem to me that it is at all an advantageous way to take the comparison spectrum across the star spectrum, so that the comparison spectrum obliterates partially or wholly the very line which has to be measured. Prof. Vogel's methods of getting over this difficulty are most ingenious; but I should myself prefer to avoid it altogether, by photographing the two spectra side by side. Then it struck me it was a complicated way of bringing the slit into the primary focus of the telescope by means of scale settings according to a temperature table. A laborious operation was necessary to determine the values of the temperature correction, and the values thus obtained are used as a setting-scale for the Potsdam spectroscope. But such an adjustment is made so very rapidly by direct visual observation in the spectroscope itself, that it seems to me it would be the better way of doing it. I think those are the only two points for criticism which the paper suggested to me.

Mr. Ranyard. I have seen a glass contact print of one of these negatives of a photograph of the spectra of Sirius and the iron spectrum, and it was very beautiful and much sharper than anything I have ever seen before. One can only hope that Prof. Vogel has not underestimated the probable error of his determinations of the motion in the line of sight. If we really have such accuracy as he believes he has attained to, we have a means of estimating the distance as well as the actual motions of the stars in the heavens; for on the average their motion in the line of sight must be equal to their motion across the line of sight which we are able to observe as proper motions; and if we knew the actual motion across the line of sight as well as the apparent motion we should know the distance of such stars. I do not quite understand Prof. Vogel's comparison of the hydrogen lines with the absorption lines in the spectrum of the Moon, for there are many times in the year and in the month when the Moon is moving away from us towards the Sun with a velocity of more than half a mile per second, and also when we are near to our mean distance from the Sun, and the radius vector of the Earth's orbit is rapidly shortening or lengthening, when there ought not to be such a coincidence of the lines.

Capt. Noble. I have consulted some comparatively recent observations of Venus in the line of sight. You will find they are within $\frac{4}{10}$ of a mile. This is a known quantity.

Mr. Maunder. One particular observation that I looked at with great interest in Prof. Vogel's paper, was his observation of Sirius. It will be within the memory of most of the Fellows of the Society that when I first began to observe at Greenwich I found a displacement towards the red for the F line in Sirius, and it seemed to decrease year after year until it became a displacement towards the blue. Prof. Vogel seems to have had the same experience, I think, but some years later. I think it is the fact that his first photographs showed a marked displacement towards the red. Now his most recent observations show a very distinct displacement towards the blue. It will be very interesting to me to know how his further observations of this star turn out, because it seems curious that his observations should show a similar cycle to that which mine had done, but falling several years later.

Mr. Ranyard. Do you remember what amount of motion his last observations corresponded to?

Mr. Maunder. Yes; about 7 or 8 miles per second of recession, as derived from comparisons both with iron and with hydrogen.

Mr. Ranyard. The change of motion can clearly have nothing to do with the companion we know of, I should think.

Mr. Maunder. No.

Mr. Ranyard. There is one point I should like to make. Prof. Vogel mentions that he compared the hydrogen-line with the line of the Moon. There ought to be a displacement with regard to the Moon. I think it ought to be visible if we can measure to a mile per second.

A vote of thanks was passed to Dr. Vogel for his paper.

Mr. H. H. Turner then read a paper on "Distribution of Temperature in the Transit-Circle Room of the Royal Observatory, Greenwich." Mr. Turner said he had to trouble the Society again with rather a technical subject, and he found it was a considerably more complicated matter when he looked into it than he expected. Perhaps he might say a word as to the object of such an investigation. With any instrument whatever which observes the heavens, it was the custom to try and reduce the air surrounding the instrument to the temperature outside. It was, of course, not possible to get the temperature of the observing-room quite down to that of the external air. It was, however, obvious that so long as the strata of air were spherical or cylindrical no disturbing effects of refraction would be noticed; for the rays of light would strike each stratum normally. The Transit-Circle Room at Greenwich was rectangular. [Blackboard illustration.] The first step taken in this investigation was to hang thermometers in different parts of the room, and find out what was the actual shape of the strata of air and what were the general possibilities; and accordingly thermometers (N, M, S) had been hung during the last three years at three crucial points of the Transit-Circle Room at Greenwich, namely, in the north and south corners and at the middle of the horizontal shutter opening in the roof. There were thus altogether 4 thermometers inside the room, one being the ordinary interior thermometer (I) near the telescope. There was also the exterior thermometer (E) just outside the North Wall, and another (F) exposed under standard conditions outside in the courtyard. The excesses of these thermometers over the standard thermometer F had been observed and tabulated during the period from January 1888 to March 1891. The general result was that these thermometers exceeded the thermometer F, and the excesses were large at small temperatures and small at high temperatures. [Details and figures on Blackboard.] There was a general decrease of the excesses from low temperatures such as those in winter to the high temperatures of summer. The observations were all made at night, and on occasions when the shutters had been thrown open for at least half an hour. Now at this point it was interesting to recur to the object of such an investigation. It seemed clear that if the internal temperature of the room had any effect on the observations, such effect should be greater in the winter than in the summer; for the excess of interior temperature was apparently greater in winter. He had therefore collected all the observations for R—D for the last ten years, and had arranged them according to the months in which they were made, and had found that there was a sensible annual variation of R—D; but that it was greater in the *summer* than in *winter*. This seemed to be a dead-lock: because if the variation depended on the excess of internal over external temperature it should be numerically greater in winter than in the summer. Further discussion showed, however, that the simple arrangement in order of external temperature did not correspond with an arrangement according to the season of the year. Without further explanation he would put down the figures resulting from an arrangement of the results *not* collected for every 5° of external temperature, *but* for each month of the year. [Table on blackboard.] It thus became apparent that the rise of external temperature from January to April was not accompanied by a *fall* in the excess of internal temperature as one might expect, but actually by a *rise*. In explanation of this fact one might conceive the excess of the internal atmosphere to be due to various causes. There were various sources of heat in the room. For instance, there were the gas-lamps, which had not been yet removed from the Observatory-room at Greenwich in favour of electric light; there was also the observer's body; and then there was the subpermanent temperature of the walls. The gas-flames and the observer's body were disturbing causes whose influence was constant all the year round; but the walls had a tendency to diminish the excess of internal temperature in the winter, and to raise it in the summer, and consequently the excess was not so great in the winter as it should be. This anomaly might be emphasized in the case of R—D observations by the fact that R—D observations were usually made soon after the opening of the shutters.

Then there was the effect of wind on the thermometers. A concise analysis showed that the excess of interior temperature was greater for northerly than for southerly winds.

Then, again, the distribution inside the room was not even symmetrical with regard to the prime vertical. The northern side of the room was always warmer than the southern side.

Thirdly, the length of time that the shutters had been open had an influence upon the thermometers; it was somewhat curious that the internal temperature was found to increase, on the average, with the time elapsed since the opening of the shutters, which, as he had said before, had an important bearing upon the variation of R − D.

In conclusion, he dwelt upon the fact that the whole science of metrical astronomy at the present day was concerned with the veriest detail; and the most careful study was necessary for any investigation dealing with the small quantities with which they were now concerned.

The Astronomer Royal. I wish to express my appreciation of the very valuable work Mr. Turner has done in discussing these points of detail, because I fully agree with him, that upon these discussions depends all the accuracy of modern observations. I daresay many Fellows are aware that a very important discussion is in progress as regards the movement of the pole of the Earth. It was alluded to by the President of the Royal Society as being securely established that the latitude of Observatories was likely to vary in the course of the year by a considerable fraction of a second of arc. The certainty of this result depends entirely upon the conditions of observation, and upon the discussion of details, such as those of the temperature of the observing-room, and other disturbing causes; and I think such a discussion as Mr. Turner has taken up is of very great value indeed. I think I have had occasion before in this room to point out that the question of determinations of position, especially in zenith-distance, is more or less a question of determining the temperature of the air; and it is all important that we should study the condition of the strata of the air immediately surrounding the telescope if we want to get any certain results. With that view we have lately had erected a new transit pavilion, intended chiefly for longitude observations, where special attention has been given to securing symmetrical distribution of temperature around the instrument; the roof has been arranged so that the telescope occupies the centre of a semicircle; and if we assume that the effect of the roof would depend somewhat upon its shape, the refracting strata would be approximately concentric semi-circles with the instrument in the centre. I hope with this observing-room we may be able to obtain results bearing upon the discussion of these questions; but I only mention this because I think it is of the utmost importance in designing an observing-room to consider the shape of the room and the influence it may have upon the strata of air immediately surrounding the instrument.

Mr. Downing. It may be worth while to point out that at Pulkowa they have gone into this question of the variation of refraction. They correct observations for the interval from the beginning of the year to the time they want the refraction taken out, and they vary the factor for every month of the year. I do not know how exactly they have arrived at their factors, but by the use of that formula they get rid of the variation of refraction, and the observations at Pulkowa are free from that variation. It is no doubt a change in the arrangement of temperature of the external air that causes it; but at all events they get over the difficulty at Pulkowa by the factor, and they appear to produce zenith distances which are free from errors of temperature of the air. It is desirable the question of temperature should be investigated in the way Mr. Turner has done it, and this paper throws additional light upon the matter.

The Chairman. It does not seem to me at all unlikely that the general shape of the atmosphere may change from summer to winter, so that the vanishing point for refraction is not at all seasons perpendicular to the surface of mercury, but shifts from summer to winter. This is not a new idea of mine; I recollect that more than one series of observations at Greenwich seemed to indicate such changes. And when you come to discuss such questions as change in the position of the axis of rotation of the Earth, I think that the possibility of such general changes in refraction from summer to winter should be taken into account.

The Astronomer Royal. In speaking of that do you mean as affected by the immediate surroundings of the instrument or as depending upon the external surface of the atmosphere?

The Chairman. Depending upon the external surface of the atmosphere on a large scale. I think it is quite possible.

The Astronomer Royal. I think, before we assume anything of that kind, my point would be that we ought to make it quite certain that the conditions in the immediate neighbourhood make it certain of this. I think we might have variations within a few feet of our instrument which might account for discordances, which we might refer to variations of temperature 50 miles off. We want to examine carefully the immediate surroundings of the instrument. It was published some years ago that by using the temperatures given by a front-court thermometer, at a certain distance from the Transit Room, instead of the ordinary exterior thermometer, a discordance which had been found in certain observations for latitude in the winter and in the summer could be made to disappear, practically, completely. We have to be very careful, before we refer to any external causes, that we are quite certain that the immediate surroundings of our instruments are free from suspicion; that we have got the true temperature of the air; and that we have homogeneous strata of air in the immediate neighbourhood.

The Chairman. I quite agree with you in that matter. The mean refraction involves an unknown quantity. It is only those differences which you have been pointing out which are exceedingly important. The differences in the second table (given by Mr.

Turner) are so small that I do not think they count for anything of importance.

A vote of thanks was passed to Mr. Turner for his paper.

Dr. Common then gave a short description of a paper "On the construction of a 5-foot Reflecting Telescope." He said: This is a paper I have written on the construction of a 5-foot equatorial telescope, which has occupied me for the last 4 or 5 years. It is absolutely impossible at this very late hour to give you the whole thing written here, but I will take one or two of the more interesting points and try to give you a condensed description of the work so far as I have gone. The paper gives an historical account of the work I did before I settled upon a regular system of work. The most interesting part of the whole of this paper is with regard to the grinding-machine with which the 5-foot mirror was made. As every one knows who has gone in for reflecting-surfaces, the enthusiasm with which you begin your work generally sticks at grinding the mirror. You do not wish to go further. The tendency is to keep on with this kind of work; and, partly for that reason, I shall wish to make this the point of the paper. I may mention that the paper which I hope you will have the opportunity of reading is divided into ten sections. I have taken first the arrangement of the workshop, than the grinding-machine and the polishing-apparatus, the mounting of the mirrors, and so on. I will indicate one or two things. With regard to the arrangement of the grinding-shop: I thought that the essential of a good grinding-shop was that it should be always clean, and I arranged to have it one gigantic sink, as large as this room, so that it could be sluiced out with water and thus kept clean. With regard to the grinding-machine, most of you know the constructions which Grubb, Herschel, Rosse, and other workers have adopted. My machine does not resemble these very much in appearance, the one that it most nearly approaches to being Grubb's, with which he made the 4-foot telescope. It enables one to turn up the mirror and examine it easily at the centre of curvature. Dr. Common then described the machine with the aid of lantern and slides, prepared by Dr. Common and by Mr. Taylor that morning. A series of curves of strokes used were also shown, but there was not sufficient time to enter into the actual construction of the telescope.

A vote of thanks was passed to Dr. Common for his paper.

The following papers were announced:—

S. W. Burnham. "The Motion of Σ 2525."

Sydney Observatory. "Observations of Comet Barnard, 1891."

Rev. W. Sidgreaves. "On the Stonyhurst Drawings of Sun-spots and Faculæ."

R. T. A. Innes. "Secular Perturbation of the Earth's Orbit by Mars."

Radcliffe Observatory, Oxford. "Observations of Comet Encke."

Royal Observatory, Greenwich. "Observations of Comet *b* 1891 (Wolf)."

Prof. H. C. Vogel. "On the Spectrographic Method of determining the Velocity of Stars in the Line of Sight."

H. H. Turner. "Distribution of Temperature in the Transit-circle Room of the Royal Observatory, Greenwich."

A. A. Common. "On the Construction of a 5-foot Reflecting Telescope."

Dr. J. L. E. Dreyer. "Note on some apparently Variable Nebulæ."

Rev. T. E. Espin. "Note on the Variability of Es.-Birm. 673=D.M.+39°, 4208.

E. J. Stone. "On the Verification of the Expressions given in Delaunay's Lunar Theory by a direct Differentiation and Substitution in the Differential Equations."

Lord Edward Spencer Churchill, Castlemead, Windsor, was duly elected a Fellow of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

Bertram Bennett, B.A., Paignton, South Devon (proposed by E. Dunkin); *Charles Bright, Assoc.M.Inst.C.E., &c.*, Telegraph Works, Silvertown, Essex (proposed by Lord Crawford); *Charles Burckhalter*, Chabot Observatory, Oakland, California (proposed by W. M. Pierson); *A. H. Molesworth, B.A.*, Barrister-at-Law, 15 Park Lane, W. (proposed by A. S. Herschel); *R. A. Simpson, B.A.*, Isaac Newton Student, St. John's College, Cambridge (proposed by A. M. W. Downing); *C. D. Webb, B.A., B.Sc.*, King's College School, and 112 Adelaide Road, N.W. (proposed by H. P. Hollis).

ROYAL METEOROLOGICAL SOCIETY.

The usual monthly meeting of this Society was held on Wednesday evening, December 16th, at the Institution of Civil Engineers; Mr. Baldwin Latham, M.Inst.C.E., President, in the Chair.

Mr. R. H. Hooker, B.A., Mr. A. B. MacDowall, M.A., Mr. E. G. Ravenstein, F.R.G.S., and Mr. R. Hedger-Wallace were elected Fellows of the Society.

Mr. W. Marriott gave the results of the investigation undertaken by the Society into the thunderstorms of 1888 and 1889, which he illustrated by a number of lantern slides. The investigation was originally confined to the south-east of England, but as this district was found to be too circumscribed, it became necessary to include the whole of England and Wales. After describing the arrangements for collecting the observations and the methods adopted for their discussion, Mr. Marriott gave statistics showing the number of days on which thunderstorms occurred at each station; the number of days of thunderstorms in each month for the whole country; the number of days on which it was reported that damage or accidents from lightning occurred; and also the number of days on which hail accompanied the thunderstorms. In 1888 there were 113 days, and in 1889 123 days on which thunderstorms occurred in some part of the country. The number of days with damage by lightning was 33 in 1888, and 38 in 1889; and there were 56 days in each year on which hail accompanied the thunderstorms. The tables of hourly frequency show that thunderstorms are most frequent between noon and 4 P.M., and least frequent between 1 A.M. and 7 A.M. Thunderstorms appear to travel at an average rate of about 18 miles per hour in ill-defined low barometric pressure systems, but at a higher rate in squally conditions. The author is of opinion that individual thunderstorms do not travel more than about 20 miles, and that they take the path of least resistance, and are consequently most frequent on flat and low ground. Detailed isobaric charts, with isobars for two-hundredths of an inch, were prepared for 9 A.M. and 9 P.M. each day for the month of June 1888. An examination of these charts showed that instead of the pressure being so very ill-defined, as appeared on the Daily Weather Charts, there are frequently a number of small, but distinct areas of low pressure, or cyclones, with regular wind circulation; and that these small cyclones pass over the districts from which thunderstorms were reported. Sometimes it is not possible to make out well-formed areas of low pressure from two-hundredths of an inch isobars, but there is a deflection of the wind which shows that there is some disturbing cause; and thunderstorms have usually occurred in that immediate neighbourhood. The author believes that the thunderstorm formations are small atmospheric whirls—in all respects like ordinary cyclones; and that the whirl may vary from 1 mile to 10 miles or more in diameter. There are frequently several whirls near together, or following one another along the same track. The numerous oscillations in the barometric curve are evidently due to the passage of a succession of atmospheric whirls; and it appears that lightning strokes are most frequent when these oscillations are numerous.

Mr. F. J. Brodie read a paper "On the Prevalence of Fog in London during the 20 years 1871 to 1890." The popular notion that November is *par excellence* a month of fog is not confirmed by the figures given by the author. The number of fogs in that month is, if anything, slightly less than in October or January, and decidedly less than in December, the last-mentioned month being certainly the worst of the whole year. The latter part of the winter is not only less foggy than the earlier part, but is clearer than the autumn months. In February the average number of days with fog is only 6·6, as against 8·9 in January, 10·2 in December, 9·2 in October, and 8·8 in November.

The Earth's Figure.

A LONG and interesting article on the Study of the Earth's Figure by means of the Pendulum, by E. D. Preston, appeared in the 'American Journal of Science' for June 1891. The following is a brief account of the paper.

The Earth's figure has been successively supposed to be a cylinder, bearing land and water on the upper base, a cube, a sphere, an oblate spheroid, and an ellipsoid with three unequal axes. Seven generations were contented with the cylindrical theory. Aristotle supposed the Earth to be spherical, and Eratosthenes computed its dimensions from observations of the Sun's shadow. In 1669 Picard measured a degree by triangulation, and since then close and closer approximations to the still unknown truth have been frequently made.

Two hundred years ago it was found that a clock keeping accurate time in Paris lost two minutes a day in Cayenne. The pendulum had to be shortened by ·1 of an inch in order to make it beat at the same rate as in a more northern latitude. But since one quarter of the Earth's circumference alters the length of the seconds pendulum only by its $\frac{1}{200}$ part, and since the elevation of the highest mountain alters the length by $\frac{1}{1500}$ part, it is evident that we must deal with minute quantities by delicate methods in all work pertaining to the measurement of the force of gravity. It is much easier to measure a small space than a small fraction of a second, yet independent observations of the time of an oscillation do not differ by more than the hundred-thousandth part of a second. Professor Harkness points out that the methods have greater power if triangulation is used only for measuring size, and the pendulum for measuring flattening. The anomalies observed in measuring local deflections from the normal force of gravity are several times as great as the hundred-thousandth part of gravity, so that nothing is gained by exceeding accuracy. "One general result seems to be that islands are heavy and mountains are light. . . . . An observer has gone so far as to say that the Allegheny Mountains weigh less than nothing; meaning by this that if gravity at the top be corrected for elevation the result is not more than gravity at the base, showing the downward attraction of the mountain to be practically nothing." When the peninsula of Hindostan was surveyed, the plumb-line was found, contrary to expectation, to be deflected towards the sea. At Dehra, the Himalayan attraction on the plumb-line is $\frac{1}{5000}$ part of the total force of gravity at the Earth's surface. These results can only be explained by supposing that the attraction of continental masses is compensated partially by a deficiency of density in the underlying strata.

Mr. Preston holds that the surface of the Earth tends to collapse into a tetrahedron. For, of all geometrical bodies a sphere has a minimum surface for a given capacity, and a tetrahedron a maximum surface: therefore a tetrahedral shape most easily relieves the tangential strains of a collapsing spherical envelope. On this hypothesis the ocean areas are nearer the centre of the globe, and therefore show increased gravity, while the attraction of such masses as the Himalayas would elevate the water surface, and gravity near a mass of land would be diminished by twice the elevation. Mr. Green finds a correspondence between actual fact and theory, by assuming that a polar sea and an antarctic continent exist, and treating Europe and Africa as one continent.

The pendulum was applied in finding the Earth's mean density at Maui, one of the Sandwich Islands, in 1887. The force of gravity was measured at the sea-level and at the highest practicable point. The result showed that a disturbing effect of 28″ must be expected in the star observations at a chosen point. The method of triangulation brought out a discrepancy of 29″. This close agreement gives confidence in the method of measuring gravity by the pendulum. The mean density of the mountain was found to be somewhat greater than that of the rocks on the surface, which is an unusual result. As mountain masses and tablelands are generally found to be light, the adopted correction for the loss of time for 1900 feet above sea-level is 5 seconds per day; the pendulum will lose 8 seconds on account of its elevation, and will only be accelerated 3 seconds by the mountain mass. At the Sandwich Islands, at a height of 10,000 feet the pendulum lost 41 seconds per day from elevation, and gained 13 seconds from the mountain's attraction. The total loss of 28 seconds agrees fairly with the adopted rate.

Mr. Preston considers that "it has been abundantly demonstrated that latitudes may have an annual variation of a considerable fraction of a second." He thinks it worth while to measure the changes of gravity in one place, for they would most probably be produced by changes of latitude. The Greenwich observations show a long period of inequality extending over 60 years*.

The small corrections needed to reduce different experiments with the pendulum to the same conditions are unimportant: they are due mainly to the buoyant effect of the air and its viscosity. Two methods have been chiefly followed in experimenting. First, noting coincidences between the gravity-pendulum and a clock-pendulum. Second, registering on a chronograph the passage of the pendulum across a fixed point. Forty of these transits suffice to give a mean value with a probable error of ·003 second of a chronographic set. When this is divided by 15,000, the number of oscillations in one swing, we get an accuracy beyond one-millionth of a second. But the first method is more accurate. For instance, suppose the gravity-pendulum loses 2 oscillations in 600 of the clock-pendulum, and that coincidence was noted after 602 oscillations. This error is $\frac{1}{300}$ of the interval, but the error in the length of one oscillation is only $\frac{1}{300}$ of the rate of gain of one

* [We cannot accept this conclusion.—Eds.]

pendulum on the other; that is to say $\frac{1}{300}$ of $\frac{1}{300}$, or about 0·00001. Professor Mendenhall arranged spots in a curve on a card fastened to the clock-pendulum; the spots disappeared at each coincidence of phase. The curve resembled a hyperbolic spiral, which would give about equal times between successive disappearances for all amplitudes of oscillation. The transit of the pendulum across the vertical thread of a telescope can be compared with the beat of a clock. An improved method is to compare a pendulum beating half-seconds, with a chronometer beating whole seconds. The chronometer opens every second the armature of a relay, to which is fastened a thin piece of metal with a slit. This slit passes in front of a fixed slit through which a light flashes on to two mirrors. One mirror is fastened to the pendulum. The mirrors are arranged so that the images of the slit are reflected into a telescope, and coincide at every coincidence interval.

Experiments have been made to find out how near a pendulum can swing to the walls of a chamber without altering its time of oscillation. All experiments in the new work proposed by the Coast and Geodetic Survey are to be made at a given atmospheric pressure, so that the pendulums must be enclosed in boxes into which air can be pumped. When the sides of the chamber were within an inch of the side of the pendulum an effect was noticed. Temperature is determined by fastening a thermometer inside a pendulum of the same size, shape, and material as the gravity-pendulum.

The computations of the force of gravity at some stations in Africa and islands in the Atlantic have just been completed, and the results agree with the theories held by Mr. Preston.

Preliminary Notice of the Reduction of Rutherfurd's Star-plates *.

Over thirty years ago Lewis M. Rutherfurd began in this city his experiments in astronomical photography. He continued his work for twenty years. No photographs were taken after the year 1877.

Previous to 1868 Mr. Rutherfurd had experimented with various telescopes, both refracting and reflecting. His best photographs up to that time were taken with a refracting equatorial corrected for chemical rays of light. This telescope was 11¼ inches in aperture and had two object-glasses, one for seeing and the other for photographing. The seeing lens was unscrewed from the tube and the photographing lens was put into its place when photographs were to be taken. This telescope was sold to Dr. Gould for use at Cordoba in the Argentine Republic. The object-glass was broken in transit. It was put together and some photographs were taken with the broken lens; but afterwards Dr. Gould had another lens made, with which he obtained many plates of the southern clusters.

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* Read at Meeting of National Academy of Sciences on November 12, 1891 New York City.

Mr. Rutherfurd devised and constructed his own measuring machine for measuring the star-plates. This was arranged to measure position-angle and distance of every star on a plate from a central star. The distance measures depended (up to 1872) entirely on readings of a micrometer-screw. With this machine Mr. Rutherfurd had measures made on many of the star-plates, among them the Pleiades and Præsepe clusters. These measures were reduced by Dr. Gould, who presented his results to the National Academy in papers read in August 1866 (on the Pleiades) and in April 1870 (on the Præsepe group). In these reductions Dr. Gould showed the great accuracy of the measures, and hence the immense value of the method for determining parallax and relative proper motions. Owing to circumstances not necessary to detail here, these papers were not published until 1889, when they appeared in Volume IV. of the Memoirs of the National Academy of Sciences.

After 1868 Mr. Rutherfurd finished a new form of photographing telescope. He wished to be able to use his telescope for seeing or for photographing without being compelled to take out the seeing object-glass. He therefore constructed a third lens which, being placed outside the ordinary object-glass, converted the telescope into a photographing instrument. The aperture of the telescope was 13 inches. Its visual focus was 15 feet 2 inches and its photographic focus was 13 feet. In his work he took account of the effect of temperature on the length of the galvanized iron tube.

This telescope, with most of the instruments in his observatory, he gave to the Columbia College Observatory in December 1883. It is now mounted in the college observatory, 110 feet above ground, and, unfortunately, within 250 feet of one of the greatest railroads in the United States.

After 1868 all the photographs were taken with this improved instrument. Mr. Rutherfurd also improved his measuring machine by using a *glass scale*, one division being equal to ten revolutions of the micrometer-screw. Thus he used the screw only for a few revolutions. He also graduated a circle on glass, which, however, he did not employ in making measurements.

The new machine was used in making all measures after 1872. This machine was also presented to Columbia College in 1883. For a number of years thereafter it was loaned to Dr. Gould, who used it in measuring at Cambridge, Mass., the plates taken in South America.

On November 13th, 1890, Mr. Rutherfurd gave his best negatives to Columbia College. This valuable collection of photographs of the Sun, the Moon, and the star-clusters has been placed in a fire-proof vault by direction of President Low. A complete list of these negatives has been prepared, and the New York Academy of Sciences has published the catalogue in its 'Annals,' vol. vi. June 1891.

This catalogue shows:—

175	plates of the	Sun	taken between	1860–1874.
174	,, ,,	Solar spectrum	,, ,,	,,
435	,, ,,	Moon	,, ,,	1858–1877.
664	,, ,,	Star-clusters	,, ,,	,,

Some of the principal star-plates may be mentioned:—

33	plates of	44 Boötis	taken between	1868–1875.
12	,,	B.A.C. 8083 (Cassiopeiæ)	,, ,,	1873–1874.
27	,,	η Cassiopeiæ	,, ,,	1870–1873.
58	,,	μ Cassiopeiæ	,, ,,	1868–1873.
15	,,	β Cygni	,, ,,	1875–1876.
24	,,	21 Cygni	,, ,,	1875–1876.
22	,,	61 Cygni	,, ,,	1871–1876.
19	,,	71 Cygni	,, ,,	1875–1876.
27	,,	Perseus clusters	,, ,,	1865–1874.
54	,,	Pleiades	,, ,,	1865–1874.
23	,,	Præsepe	,, ,,	1865-1877.
23	,,	1830 Groombridge	,, ,,	1872- 1874.

When this collection of negatives was turned over to the observatory of Columbia College it was arranged with Mr. Rutherfurd to have the work of reduction of the measures of the star-plates pushed forward as rapidly as possible. At my suggestion Mr. Harold Jacoby of the Observatory undertook the reduction of the Pleiades plates taken with the 13-inch and measured with the improved machine.

These reductions are now almost completed and seem to indicate an accuracy of measures comparable with the best recent heliometer work. The Observatory will publish very soon, in the Annals of the New York Academy of Sciences, a complete account of these reductions as well as a description of the instruments used and methods employed. Our desire is to go forward in these reductions until we have finished all the measures made by Mr. Rutherfurd with the improved machine. These measures fill some twenty folio volumes of about two hundred pages each. Then when all the measures are reduced we may be able to measure the many negatives that remain unmeasured and proceed to their reduction.

The members of the National Academy of Sciences thus are made aware that the college in which Mr. Rutherfurd was for twenty-six years an honoured and most efficient trustee is determined to put into useful and permanent shape the star work of their eminent colleague, who did so much to originate means and methods, and gave most invaluable direction and impulse to astronomical photography.

J. K. Rees.

Remarks by Dr. B. A. Gould.

It affords me especial pleasure to know that these long kept measurements of the earliest photographs available for astronomical determinations are now to be made available for scientific use. This college, which Mr. Rutherfurd held in such affection, and to which he has presented all his astronomical appliances—unsurpassed in themselves and now endowed with exceptional historic value—is the fittest place for the computations to be carried out; and I am confident that the work will be well done under the direction and guidance of Prof. Rees and his assistant, Mr. Jacoby.

Mr. Rutherfurd was the originator and the introducer of the photographic method of observation. To him is due the first idea and employment of an object-glass constructed for employing the chemical rays rather than the visual ones; as also, later, that of the "photographic corrector" for adapting an ordinary object-glass to its best use in securing sharp definition of the stars upon the sensitive plate. He personally planned the construction of the first instruments of each of these classes, prescribed the curves for the several surfaces of the lenses, and superintended the preparation of the object-glasses, which were made, with Mr. Fitz's assistance, in his own house, by methods devised and made practical by himself alone. So, too, was it he who introduced the precautions by which the sensitive film was guarded against distortion; it was he who first devised and constructed micrometric apparatus for measuring the impressions upon the plates; and he who first put this apparatus into practical use in executing the measurements. The large and delicate micrometer-screws were made by him, or under his constant supervision, at his dwelling-house in this city, and the measurements were effected in his study.

It was my fortune to know of his work in this field at a comparatively early day, and to be able to follow it closely, with sympathy and admiration. Hence I cannot refrain from bearing my testimony to the essential part which he bore in introducing and establishing the photographic method of observation. For many years he laboured at it without the sympathy or encouraging faith of astronomers generally; and in 1865 he did me the honour of placing in my hands a large number of measurements, and giving me permission to study and compute them. They had been made in his house, with apparatus designed and, in great degree, constructed by himself, from photographs which he had personally taken by the aid of the telescope which he had himself devised and which was also in his house.

At the session of this Academy at Northampton in August, 1866, I had the pleasure of presenting a memoir containing the results of computations, made from these data, for determining the relative positions of 31 stars in the Pleiades. On the same day Mr. Rutherfurd communicated to the Academy orally a detailed account of his experiments, difficulties, and successes, as well as of the methods which he had finally adopted. It was his expressed intention to put this communication into writing at once, for publication as one of our memoirs. And I naturally delayed the publication of my own communication until it could be preceded by that of which it was essentially a follower, and which described the mode of obtaining the data upon which my own subsidiary memoir depended. The only step which I took towards publication was by mentioning the investigation in a letter sent during the same month to the 'Astronomische Nachrichten.' This contained the resultant distances and position-angles from *Alcyone* for the brightest ten stars of the group, and called attention to their close accordance with Bessel's earlier values, deduced from his observations with the Königsberg heliometer.

All this I am glad to recall here and now, Mr. President, because, from various statements made at far more recent dates, and the comparative newness of the interest in photographic observation now manifested by astronomers, one might easily be led to suppose that the method had been recently introduced, or, indeed, hardly existed prior to the immense facilities afforded since 1880 by the dry-plate gelatine process. In this connection I may be excused from adding that the knowledge acquired from Mr. Rutherfurd of the important methods which he had developed had so aroused my enthusiasm for photographic observation, and my faith in its future application, that when I left this country for South America, in May 1870, I took with me the photographic object-glass with which his results had been obtained. The corrector for his new 13-inch telescope was then essentially completed; so that he kindly consented to part with its smaller predecessor.

The continued and disheartening obstacles which I encountered in the endeavour to secure photographic impressions of the clusters in the southern sky are known to many of the gentlemen present. Still, at the close of the year 1876 I had obtained nearly 500 satisfactory plates, and in November, 1882, there were 1100. A large part of these have already been measured, and the Argentine Government has now, in spite of its financial embarrassment, assigned me the means for resuming the computation.

Unless I am mistaken, all of the plates of which Prof. Rees has spoken were made some years before the discovery of the dry-plate process. Indeed by far the greater number of the impressions must have been taken previous to 1875. In later years the gelatine process has wonderfully facilitated the prosecution of stellar photography, making the duration of the necessary exposure very much less, while the limit of stars photographed is lowered to an almost incredible extent. And since there is now practically no limit to the time of exposure, which may extend over all the hours of darkness for many successive nights—whereas it was previously restricted by the rapid drying of the plate—the positions of stars which are beyond the reach of powerful telescopes may be fixed at present by photographic methods. It is easy, to-day, to secure good impressions of ten or twenty times the number of stars, during an exposure of given length; and easy to extend the time of exposure to fifty times that which was possible by the photographic processes known previous to 1880.

Still we must not forget that the simple reproduction upon the plate of an image of any part of the sky is not astronomical research, but simply an attainment of a means for its convenient application. A photographic plate gives us no more than can be seen by any one using adequate visual appliances, and it is with the careful measurement of the plates that the astronomer's work begins. Here Mr. Rutherfurd's services have, as I have said, been as effective as in the collection of material; so that we owe to him not merely the first permanent records of the relative positions, at a given moment, of all the celestial objects impressed upon the sensitive plates, but the means and the accomplishment of the actual conversion of these records into actual numerical data.

How greatly I have rejoiced at knowing that these numerical data, which have so long slumbered undisturbed, are now to be computed and given in due form to the astronomical world, it would be impossible to say.

Not long after the session of the Academy, in August 1866, to which I have referred, Mr. Rutherfurd was obliged, by impaired health, to leave home for a considerable period; and this, together with various duties which awaited his return, prevented him from writing out his memoir, which, unhappily, has never been published.

Meanwhile, I computed the positions of more than thirty stars in the *Præsepe*, precisely in the same way as those of the *Pleiades*, yet retained the results in my own possession for the same reason.

When, however, it became necessary for me to leave home for South America, in May 1870, I communicated these positions to the Academy at its April session of that year. But I still withheld the manuscript of the two memoirs from the press, deeming it entirely improper that their publication should antedate Mr. Rutherfurd's description of the various methods by which he had obtained the fundamental data. And when I left home for an absence which, as I supposed, would not exceed three years, I handed the two manuscript articles to Mr. Rutherfurd himself, with an injunction that they should not be given to the Home Secretary for publication until his own memoir should accompany them.

For the whole period of my long absence the papers remained untouched; and when they first saw the light they were printed from the original manuscripts, without emendation and during my absence from the country, in the summer of 1888, being twenty-two years after the identical manuscript of the first, and eighteen years after that of the second, had been presented to the Academy.

In the meanwhile various deficiencies in the method of computation, and even one or two errors, had come to my knowledge. But for the sake of the history I preferred that these should remain, rather than that the memoirs should not be printed exactly as they had been originally prepared, and without correction. Of course I was unable even to revise the proofs, being in Europe at the time of their printing.

During this lapse of a score of years, improvements in the mode of computation, and of the presentation of the results, have naturally suggested themselves; and it is a welcome thought that under the direction of Prof. Rees, and his assistant, Mr. Jacoby, additional precautions and better methods will unquestionably be employed.

Double Stars for January.

The following double stars are well placed for observation in January. They may all be observed with instruments of moderate aperture. The positions are for 1890.

ζ Orionis (Σ 774). R.A. $5^h\ 35^m$, Dec. −2° 1′; Mag. 2·0, 5·5.

1882·10	153°·05	2″·66	Bigourdan.
1889·04	155 ·3	2 ·80	Leavenworth.

Σ 948 = 12 Lyncis. R.A. $6^h\ 37^m$, Dec. +59° 33′.
Triple 5·7, 6·4, 7·4.

5·7 and 6·4 form a binary pair. Period 485·8 years (Gore)*.

AB	1887·32	126°·32	1″·56	Tarrant.
½ (A+B) and C	1887·32	306 ·62	8 ·62	Tarrant.

The three stars are now in a straight line.

Σ 1037. R.A. $7^h\ 6^m$, Dec. +27° 25′; Mag. 6·9, 7·1.

Slow motion.

1881·12	313°·6	..	Bigourdan.
1887·18	310 ·9	1″·26	Tarrant.

δ Geminorum (Σ 1066). R.A. $7^h\ 14^m$, Dec. +22° 11′;
Mag. 3·5, 9.

Slow direct motion.

1880·22	204°·5	7″·26	Bigourdan.
1886·15	207 ·20	6 ·98	Tarrant.

Castor. R.A. $7^h\ 28^m$, Dec. +32° 8′; Mag. 2·0, 2·7.

A famous binary star. Period still somewhat uncertain.

1888·28	230°·9	5″·85	Schiaparelli.
1889·09	229 ·6	5 ·68	Leavenworth.

* [A later measure by Mr. Maw (see "Companion") gives
AB 1889·31 124°·9 1″·57 Eds.]

Σ 1126. R.A. 7^{h} 34^{m}, Dec. +5° 29′; Mag. 6·8, 7·2.

Slow direct motion.

1889·04 142°·7 1″·47 Leavenworth.

Σ 1157. R.A. 7^{h} 49^{m}, Dec. −2° 29′; Mag. 8, 8.

1887·71 247°·08 1″·27 Tarrant.

J. E. Gore.

Selenographical Notes.

Pitatus and Hesiodus.—Though the borders of both these formations are narrow and in places discontinuous on the north, where they face the Mare Nubium; unlike those of Fracastorius, Doppelmayer, and other rings similarly circumstanced with respect to the "seas" in their neighbourhood, the gaps are nowhere wide, the existing walls are of considerable steepness and height, and, being easily traceable under all conditions of illumination, neither of them resembles a bay-like inflection in the coast-line as do the objects just mentioned. The lowest section of the ring of Pitatus extends for a distance of ten or twelve miles east of the conspicuous crater C (marked G in Neison's Map XIV.) on the north border; but even here some detail, consisting of small isolated hills and a minute crater, is visible. Beyond this, up to the lofty mountain β, overlooking the narrow opening which connects the formation with Hesiodus, its continuity is only interrupted by two or three little breaks and it ultimately attains considerable altitude, increasing gradually in height as it approaches β.

The most impressive feature in connection with Pitatus, when viewed under oblique light, is the row of great depressions which, commencing on the north with the large irregularly-shaped crater g on the west border, runs in a southerly direction up to the lofty mountain-arm extending from the north-west wall of Gauricus; if indeed it may not be regarded as forming a southern extension of the linear row of great depressions which traverses the Tycho highlands from Sasserides, through Ball, to this arm, the direction of which (it is worth noting) is further prolonged in the form of a light streak across the Mare Nubium as far as Nicollet. It is best seen under morning illumination when the E. long. of the terminator is from 18° to 20°, and is a fine object at this phase even in a 2½-inch achromatic. There is a very similar, but somewhat shorter row adjoining these depressions on the south-east, the most northerly member of which lies close to the crater d on the west wall. A short distance north-west of the border, just on the edge of the bright surface bordering the Mare, are the remains of an old ring, f. Its broken walls and the bright crater near its centre are easily seen at this phase.

The south-west side of the formation exhibits a feature which strongly reminds one of the bright rectangular area under the attenuated north-west border of Gassendi, between the craters A and C. It is similar in shape and is bounded on the south-west by a very low and narrow extension of the border, and is separated from the floor by a row of six or seven bright craterlets. The enclosed surface has a bright silvery lustre. In connection with the west wall, Schmidt draws a curved rill-valley between *g* and *d*, of which I had a fine view, 1883 February 16 8^h, when the E. long. of the morning terminator was about 22°. It has been found less easy to trace it under a lower Sun, for on April 17 this year at 8^h 30^m it could only just be made out as a known object. The curious crater-row on the broad south-east border is also more obvious at a somewhat late stage of sunrise, say when the terminator has advanced as far east as Cichus, as under very oblique light it is masked to a great extent by the shadows of innumerable details on a slope which is almost as intricate as that of the west wall of Petavius.

The narrow pass between Pitatus and Hesiodus, overlooked by the lofty mountains β and γ, deserves careful examination under all conditions. The gap is filled with shadow for some time after the two interiors are practically in full sunshine, and I have more than once remarked this band of shade prolonged, in the form of a long black line, for some little distance on to the floor of Pitatus, as if there was a coarse cleft traversing the pass and extending beyond it towards the south-west.

There are two well-known clefts in the interior of Pitatus, ϕ and ξ, forming apparent chords to short segments of the north-east and north-west walls respectively. The first was overlooked, as Neison remarks, both by Mädler and Lohrmann; but this may perhaps be accounted for by the fact that it is obscured by the shadow of a long low mound till sunrise is well advanced. As regards ξ, is it a true rill or the clean-cut shadow of a fault or sudden drop in the floor? Schmidt does not show it. I have frequently noted it as a black line, but have never yet seen a trace of sunlight on its inner slope if it is a cleft. At 9^h on April 17 this year the northern end of it was very distinct, and I fancied that it was faintly illuminated on its western flank, as would be the case if it were a low ridge. On February 16, 1883, 8^h, a well-marked dusky streak was easily traceable on the floor, extending from a gap in the north wall, near the W. end of rill ϕ, to the west side of the central mountain A. Though it has not been observed since, there is so much that is at presnt inexplicable in connection with rills and rill-like features that the observation is placed on record for what it is worth.

In addition to the conspicuous central mountain A there are several objects on the floor of Pitatus, among them two large but very low elevations between A and the south border, and a third of a similar character on the north-eastern quarter of the interior. There are also two mounds near the pass into Hesiodus, and three or four large and prominent hills on the southern edge of the floor which are well shown by Schmidt.

It has been thought remarkable that Mädler does not show the crater near the centre of Hesiodus. Schröter in one of his two drawings of the formation represents a bright white mountain on the site, while in the other he exhibits no detail whatever. These anomalies find, I think, an easy explanation in the fact, that, though under a rising sun the mountain is obvious enough, at and about lunar noon it is a faint object, and to see it as a crater it must be observed about the time when the morning terminator has reached Cichus and the neighbourhood. THOS. GWYN ELGER.

Beaumont House, Shakespeare Road,
Bedford, 1891, Dec. 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Dark Spots on Jupiter.

GENTLEMEN,—

As Mr. Denning has referred, in your September issue, to the appearance of dark spots on Jupiter's North Temperate Belt, perhaps the following remarks, from observations made with the 8-inch refractor of the Adelaide Observatory, may be of interest.

The spots were first seen here on August 5, the weather for some considerable time before that date having been very unfavourable for telescope work. On that night they were described as appearing "absolutely *black*," and while the greater number were small round spots, one or two appeared as very short narrow streaks. Since August 5 spots have been seen on this belt on every night when the definition has been fairly good, but most of them require favourable atmospheric conditions in order that they may be seen at all and are quite invisible on nights of confused definition.

The general appearance is that of small dark spots—not black—more or less diffused at their edges and of much the same colour as the North Temperate Belt itself, though of much darker shade. Their aspect, however, appears to depend a good deal on the state of the atmosphere. For instance, one night when the definition was variable the same spots were seen sometimes as small black specks and sometimes as larger diffused spots of a dark red-brown colour.

Of late they seem to have increased somewhat in size, though they may perhaps have decreased in number, and one or two were very conspicuous the last time observations were made here.

A number of central transits has been taken, but identification of individual spots is very difficult. However, from passages of what seems to be the same spot—one of the largest of them—on October 8 at $9^h\ 42^m$ A.M.T. and Oct. 18 at $8^h\ 35^m$, the rotation period seems to be about $9^h\ 57^m$, very different from the $9^h\ 48^m$ of the 1880 spots referred to in Mr. Denning's letter. It is noteworthy also that a small but conspicuous spot was observed at central transit on September 10 at $15^h\ 3^m$. If this may be the same spot as the one mentioned above, the period from Sept. 10 $15^h\ 3^m$ to Oct. 8 $9^h\ 42^m$ allows of exactly 67 revolutions of $9^h\ 57^m$. However, on plotting the spots down on a chart in their proper longitudes, the longitudes running across, and the nights of observation down, the sheet, hardly any connection can be made out between spots or groups of spots seen on different nights, and the large spot seen on October 8 and 18 is the only one which can be identified with any degree of certainty.

While nearly all these spots are on the southern edge of the North Temperate Belt, some project largely into the bright zone to the south, while others are wholly immersed in the belt itself. Good sets of latitude measurements have been obtained here on three nights, the mean result, corrected for the elevation of the Earth above the plane of Jupiter's equator, giving $+18^\circ{\cdot}2$ as the zenographical latitude of *the southern edge* of the belt.

Adelaide Observatory,
South Australia, 1891, Nov. 11.

Yours faithfully,
E. J. Sells.

Periodical Comets due in 1892.

Gentlemen,—

Of all the periodical comets which are known to have been actually observed at two or more returns, only one is due to return in 1892—that often called Winnecke's, because it was after its rediscovery by Prof. Winnecke in 1858 that it was recognized by actual return to be moving in an orbit of short period. It appears to have been first discovered by Pons on the 12th of June, 1819, and although Encke calculated that its period was about five and a half years, six unobserved returns took place before it was redetected by Winnecke (who at first supposed it to be a new comet) on the 8th of March, 1858. At the next following return, which must have taken place near the end of 1863, it escaped observation; but it was seen in 1869 (first by Prof. Winnecke himself on April 9), and in 1875, when it was detected by M. Borrelly at Marseilles on the 1st of February, and passed its perihelion on March 12. In 1880, being unfavourably placed, it again escaped observation; but in 1886 it was detected by Mr. Finlay at the Cape of Good Hope on the 9th of August, and passed its perihelion on the 16th of September. Another return, therefore, will be due early next year.

It is well known that some investigations made by the late Prof. Oppolzer, of Vienna, in 1880 seemed to make it probable that the period of this comet was undergoing a diminution similar to that which Encke had found for the comet called by his name. His suggestion that the acceleration was produced by a resisting medium retarding the onward progress of the comet and thus reducing its mean distance, long seemed plausible until the calculations of von Asten, continued since his death by Dr. Backlund, showed that a considerable diminution in the amount of change took place about twenty-five years ago. With regard to the Pons-Winnecke comet, Herr von Haerdtl, of Vienna, discussed its motions again in 1888 (another observed return having taken place in 1886 subsequent to Oppolzer's investigation), and found that no acceleration whatever of the mean motion was shown. It is to be hoped that astronomers will be able to secure a good series of observations of this comet at the forthcoming return in 1892.

It may be added that it seems not unlikely that the comet is identical with one discovered by Pons in February 1808, which was insufficiently observed for determination of its orbit at that return.

It has been calculated that another small comet which was discovered by Mr. W. R. Brooks at the Red House Observatory, Phelps, N.Y., on the 22nd of May, 1886, has also a period of about five and a half years, and will probably return in the course of 1892. Reference was made in the last number of the 'Observatory' (vol. xiv. p. 427) to Dr. S. Oppenheim's recent determination of its orbit, with the result that the most probable length of the period is 5·5954 years, which would bring the next perihelion passage to the 13th of January, 1892. It may, however, prove to be somewhat longer than this, and Dr. Oppenheim thinks that the comet will be more likely to become visible if the perihelion passage does not take place until the summer or autumn of 1892.

Yours faithfully,

Blackheath, 1891, Dec. 7. W. T. LYNN.

The Lunar Eclipse of B.C. 331, *and its bearing on Ancient Chronology.*

GENTLEMEN,—

Mr. Stockwell has recurred to the subject of ancient chronology in the 'Astronomical Journal.' There is no need to trouble you with any further detail about this, but one point which he brings forward in support of his views so completely disproves them, and so fully confirms the received chronology, that it may be worth while to refer to it.

Several of the ancient historians mention a total eclipse of the Moon which occurred in the year of the battle of Arbela or Gaugamela. Plutarch says that it was eleven days before the battle, and Arrian tells us that Alexander crossed the Euphrates in the previous Attic month of Hecatombæon. That the eclipse was the one which took place on the 20th of September (so that the battle was fought on the 1st of October) in the year B.C. 331, there can be no doubt, nor does Mr. Stockwell dispute it. But he thinks that the murder of Darius took place in the following winter; and as it is stated to have been in the third year of the 112th Olympiad, he contends that the first year of that Olympiad, when the festival was celebrated, fell in B.C. 333, which would correspond to a leap-year in Julian chronology. We are informed, however, by Arrian (lib. iii. c. 7) that the death of Darius took place in the summer after the battle, as it was perpetrated in the month of Hecatombæon. This was the very month, it will be recollected, in which the Olympic games were held, and that being in the third year of the 112th and in the summer of B.C. 330, the festival of the first year of that Olympiad must have fallen in the summer of B.C. 332, the year after a bissextile in the Julian calendar. This fully agrees with my contention that the first year of the first Olympiad fell in B.C. 776 of our chronology, as had been sufficiently proved by the authors of 'L'Art de vérifier les Dates.'

Yours faithfully,

Blackheath, 1891, Nov. 26. W. T. LYNN.

Prof. Piazzi Smyth's Levels.

GENTLEMEN,—

Having failed to find in the Library R.A.S. any clue to the form of level which, at the November meeting, I attributed to Professor Piazzi Smyth at the Exhibition of 1851, I addressed him for information. It may save others some trouble if I give you the substance of his reply.

The instruments were exhibited at the Paris Exhibition of 1855 and not in London in 1851. Also the fluid used was chloroform, as proposed by Mr. Brett, which was found very sensible.

I trust the slip in my memory of matters now nearly 40 years old will be excused.

11 Clifton Gardens, Maida Hill, W. Yours faithfully,
1891, Dec. 17. J. F. TENNANT.

Jupiter.

GENTLEMEN,—

With your permission I should like to make a rather important correction relative to the conjunction of a spot on Jupiter with the great red spot, which occurred about the middle of last year. According to your Report of the November Meeting of the R.A.S. ('Observatory,' No. 182, p. 397) it appears that Capt. Noble, in kindly referring to my observations of this phenomenon, stated that the former spot went below the latter and reappeared on the other side. In his interesting observations of Jupiter in 1890 ('Observatory,' No. 180, p. 332), Professor C. W. Pritchett also refers to the spot as emerging after having passed under the Red Spot. Probably this impression originated from my letter in the 'Observatory,' No. 165, p. 275. But additional observations obliged me to considerably modify the view there expressed, that the dark spot had passed beneath the red spot. It now appears certain, that, so far from this being the case, the material of the dark spot was diverted and forced bodily southwards, and obliged to pass round the southern side of the latter, as if it were an island projecting above a stream. This view is indicated strongly by the independent observations of several observers.

From your interesting account of the discussion that took place at the November Meeting of the R.A.S., it would seem that considerable opposition was made to the plastic theory of Jupiter's surface advocated by Mr. Barnard. Yet many of the phenomena observed on the planet undoubtedly strongly suggest a plastic condition. With regard to this I should like to ask those better qualified to give an opinion, whether large gaseous or vaporous masses might not behave to a certain extent as if they were in a plastic condition, should they be under the influence of great pressure. Mr. Ranyard speaks of a satellite having been seen through a depth of 1000 miles of Jupiter's atmosphere. A little calculation as to the probable pressure existing at the bottom of an atmosphere 1000 miles deep on Jupiter will show that it must be enormous. Intense heat would doubtless suffice to keep the constituent material in a purely gaseous or vaporous state even under very great pressure. But is the heat of Jupiter likely to be intense enough?

Yours faithfully,

Burgess Hill, 1891, Dec. 24. A. STANLEY WILLIAMS.

A New Belt on Jupiter.

GENTLEMEN,—

In reply to Dr. Swift's question in your last issue relative to a faint belt on the equatorial region of Jupiter, I beg to say there have come into my possession, as Director of the Jupiter Section of the British Astronomical Association, from W. E. Jackson, Esq., of Constantinople, 43 accurate delineations of the Jovian surface, nearly all of which show a faint equatorial marking or belt. The belt is sometimes disrupted by light or dark spots or patches lying nearly parallel with the axis of Jupiter. Mr. Jackson uses a 6-inch O.G., equatorially mounted, and is an experienced observer. Other members of the Jupiter section show occasional traces of the above belt when using adequate apertures. With the writer's 12½-inch speculum it is detected when atmospheric conditions are highly favourable. That this faint belt is not wholly new is shown by Professor Keeler's exquisite drawings in the Pacific Astronomical Society's Publications, no. 11 of vol. 2, taken with the 36-inch O.G. at Mount Hamilton during July

1889, and also the drawing in the 'Monthly Notices' of the R.A.S., no. 1, vol. 51; date of drawing Aug. 28, 1890.

Portland, 1891, Dec. 7. W. R. WAUGH.

GENTLEMEN,—

Though I did not see the belt noticed by Mr. Swift, 1891, Sept. 3, on referring to my own and other drawings of the planet, I find many sketches in which these features are represented on or very near the equator. During the very favourable opposition of 1873, a well-marked dusky band was often noted in this position. During the years 1880 and 1881, a more or less faint belt, sometimes straight, at others wavy, was frequently observed on the equator, where it generally marked the northern limit of the curious loops or festoons then associated with the south equatorial belt. In November 1881, a very attenuated but distinct band of a clear coppery hue was constantly seen only very slightly north of the equator, and on three occasions in Feb. and March 1884 (notably at 10^h Feb. 21) a dusky belt was recorded, which, so far as the eye could judge, was exactly equatorial.

Yours faithfully,

Bedford, 1891, Dec. 21. T. GWYN ELGER.

PUBLICATIONS.

COLLECTION DE MÉMOIRES RELATIFS À LA PHYSIQUE, PUBLIÉS PAR LA SOCIÉTÉ FRANÇAISE DE PHYSIQUE.—The Physical Society of France deputed M. C. Wolf to collect and publish the principal memoirs relating to the pendulum. The two volumes before us represent the outcome of this long and laborious work. First of all is an introduction by M. Wolf, giving a clear and concise *résumé* of the progress made since the time of Galileo both in the theory and practical application of the pendulum. Next is a copious and so far as we can ascertain, without duplicating the author's labours, complete list of works bearing on the subject from 1629 to 1885. Finally we are given the chief memoirs relating to the pendulum.

In the historical introduction M. Wolf contends that the history of the pendulum really commences with Galileo's discovery of the isochronism of its vibrations in 1589 and the establishment of its laws of motion in 1629, and gives to Galileo and Huyghens the whole credit of applying it to horology. He acknowledges gratefully that his chronological list owes much to Major Herschel's "Bibliographical List of Works relating to the Pendulum Operations in connection with the Figure of the Earth."

The list itself is most comprehensive and M. Wolf must literally be saturated with historical knowledge of the pendulum, for in the majority of cases he has given an idea of the contents of the memoirs, pamphlets, notes, &c. It commences with Galileo's works published in Paris 1629 and extends to the end of 1885. Of the third section we have no occasion to speak, the memoirs are well chosen and will form a perfect treasure for persons looking up the subject. We can only give a list:—

Experiments made in 1735 by M. de la Condamine in measuring the length of the seconds pendulum at St. Domingo.

Experiments for determining the length of the simple pendulum beating seconds at Paris, by MM. Cassini and Borda, 1792.

On a method of determining the length of a simple pendulum beating seconds, by M. de Prony, 1792. (This memoir has only recently been unearthed by Major Defforges.)

Experiments made to determine the length of the seconds pendulum in the latitude of London: Capt. Henry Kater, 1818.

Researches on the length of the simple pendulum beating seconds: 1826, F. W. Bessel.

On the reduction to a vacuum of oscillations of an invariable pendulum: Capt. E. Sabine, 1829.

Researches on the correction of a pendulum for the reduction to a vacuum: F. Baily, 1832.

The effect of internal friction of fluids on the motion of a pendulum: G. Stokes, 1852.

Construction of a pendulum of symmetrical form: F. W. Bessel, 1849.

Illustrations of the pendulums and fittings make the whole very complete, and it is with great pleasure that we are able to testify to the excellence of M. Wolf's work and offer him our warmest congratulations on the accomplishment of an arduous undertaking.

T. L.

THE PARIS CATALOGUE.—The second instalment of this publication has recently been issued from the Paris Observatory. This volume contains the reduction of observations of 7538 stars within the limits of R.A. 6^h to 12^h made at Paris in the years 1837 to 1881. Following the scheme of the first volume, the place of each star is reduced to one or more of the epochs 1845, 1860, or 1875 according to the years in which the observations were made.

One cannot but be struck with the large amount of work that has been done in the way of comparison. All the places of the catalogue have been compared with Lalande's places as reduced by Baily, and the differences are given in the catalogue itself. A similar comparison with the Auwers-Bradley catalogue is given in the Introduction. Comparisons have also been made with the places of many other catalogues; and here the present volume has the advantage over its predecessor, in that several catalogues published recently, notably those planned by the Astronomische Gesellschaft, have been available for this purpose. From some of the differences given by these comparisons, the proper motions of 350 stars have been deduced. Other of the differences have led to the detection of large errors in the Lalande catalogue, or have increased the accuracy of the present work.

This volume, as was the first, is produced under the direction of Mons. A. Gaillot, who acknowledges the help of MM. Bossert and Bigourdan, and who is to be congratulated on the completion of the second part of a work which has already proved so acceptable to astronomers. H. P. H.

EXPERIMENTS IN AËRODYNAMICS*.—Prof. S. P. Langley, like several other American professors, has lately turned his attention to the subject of flying-machines. A most interesting review by Lord Rayleigh of the present memoir appears in 'Nature' for Dec. 3, and although the subject is only indirectly connected with astronomy, we may perhaps give a few extracts from this review.

"The laws of resistance (to bodies moving in air) were fairly well established many years ago, at least in their main outlines. Nevertheless there was ample room for the systematic and highly elaborate experiments recorded in this memoir. The work appears to have been executed with the skill and thoroughness which would naturally be expected of the author, and will doubtless prove of great service to those engaged upon these matters. . . . The main problem is of course the law of obliquity." Prof. Langley's curves show results "anticipated in a general manner by Wenham and Froude. They demonstrate that in proportion to area a long narrow wing is more efficient as a support than a short wide one, and that in a very marked degree. . . . When once the law of obliquities is known, the problem of aërial maintenance presents no further theoretical difficulty. It was successfully treated many years ago by Penaud. . . In perhaps the simplest form of the question the level is supposed to be maintained with the aid *e. g.* of screw propulsion, the necessary maintenance being secured by an aëroplane slightly tilted (at angle α) upwards in front. The work required to be expended in order to maintain a given weight depends upon the area of the plane, the inclination, and the speed. Penaud's results show that, *if skin friction could be neglected*, the necessary work might be diminished indefinitely even with a given area of wing. For this purpose, it would only be necessary to increase the speed and correspondingly to diminish α. But when friction is taken into account, the work can only be reduced to a minimum, and to do this with a given area of wing required a definite (large) velocity and a definite (small) inclination. The accurate determination of the tangential, as well of the normal, force experienced by an inclined plane is that of essential importance in the question of flight.

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* Experiments in Aërodynamics by S. P. Langley. "Smithsonian Contributions to Knowledge" (Washington, 1891).

"The work of Penaud seems to be so little known that it has been thought desirable to recapitulate some of his theoretical conclusions. But we owe to Penaud not merely sound theory, but the actual construction of a successful flying-machine, in which horizontal flight is maintained by a screw propeller. In these models the energy is stored by means of stretched india-rubber, a method available only upon a small scale. It is probable that the principle of the rocket might be employed with advantage; and even upon a large scale the abolition of all machinery would allow of considerable extravagance in the use of explosive material.... It is interesting to note that Prof. Langley's experience has led him to take a favourable view of the practicability of flight upon a large scale. Such was also the opinion of Penaud, who (in 1876) expresses his conviction 'that in the future more or less distant, science will construct a light motor that will enable us to solve the problem of aviation.' But sufficient maintaining power is not the only requisite; and it is probable that difficulties connected with stability, and with safe alighting at the termination of the adventure, will exercise to the utmost the skill of our inventors."

NOTES.

Diameters of the Sun and of Venus.—We referred last month (p. 431) to a paper by Dr. Auwers, giving the results for Solar parallax from the German Transit of Venus expeditions in 1874 and 1882. This paper was speedily followed by another (A. N. 3068) on the diameters of the Sun and Venus as determined by the heliometer measures made in connexion with the expeditions by 31 observers with 4 instruments. The mean result for the Sun's mean diameter is 1919″·3, which differs considerably from that at present adopted in the various Ephemerides, that in the B. J. being 1922″·4, in the C. T. and N. A. 1923″·6, and in the American N. A. 1924″·0. Dr. Auwers remarks that if the value he finds is affected by irradiation effects, it can only be too large, and thus the errors of the adopted diameters will be larger still. He urges that a change should be made in these adopted values as soon as possible, and announces that for the 'Berliner Jahrbuch' the change will be made in the volume for 1895. The personal equations of the various observers in measuring diameter range from +1″·03 to −0″·88. Three of them exceed +0″·50 and six are less than −0″·50; these groups being separated with some distinctness from the others and regarded as possibly anomalous by the author.

The observations are discussed for possible ellipticity of the Sun's disk, with the result that the polar diameter is found to exceed the equatorial by 0″·032. This excess is of course different for different observers, being positive in 28 cases out of 45; but Dr. Auwers considers it due to a general tendency to estimate vertical diameters larger than horizontal ones.

We do not know whether he noticed the rather curious result obtained by arranging the observers in order of their diameter-personality for discussion of the ellipticity, as follows:—

	General Excess of Diam.	Polar—Equat.		General Excess of Diam.	Polar—Equat.
	″	″		″	″
Hartwig	+1·03	+0·08	Ambronn	+0·02	+0·10
Seeliger	+0·73	+0·13	Marcuse	0·00	+0·13
Valentiner	+0·48	+0·14	Bauschinger	−0·06	−0·05
Wittstein	+0·35	+0·05	Adolph	−0·14	+0·05
Weinek	+0·35	+0·15	Kempf	−0·14	+0·15
Schur	+0·29	+0·18	Küstner	−0·20	+0·02
Wislicenus	+0·20	−0·12	Peter	−0·33	+0·07
Kobold	+0·16	0·00	Müller	−0·33	+0·19
Auwers	+0·15	−0·01	Börgen	−0·76	+0·06
Franz	+0·07	−0·10	Deichmüller	−0·84	−0·05
Battermann	+0·05	−0·48			

It is thus apparent that the positive value for P.—E. is entirely due to those observers with large personalities. If we take only such observers as have a personality in observing diameters numerically less than 0″·25, the simple mean of the values for P.—E. is —0″·028, and the weighted mean —0″·016, so that the polar diameter would be slightly *less* than the equatorial.

The value for diameter of Venus is 16″·80. The personality in estimating this diameter is found to be accordant with that for the Sun, as shown in the following table:—

Observer, 1874.	Venus.	Sun.	Observer, 1882.	Venus.	Sun.
	″	″		″	″
Valentiner	+0·40	+0·48	Müller	−0·62	−0·33
Adolph	+0·04	−0·14	Deichmüller	−0·66	−0·84
Börgen	−0·36	−0·76	Franz	+0·17	+0·07
Schur	+0·14	+0·29	Hartwig	+0·02	+1·03
Seeliger	+0·12	+0·73	Auwers	+0·43	+0·15
			Küstner	−0·17	−0·20

THE ROYAL SOCIETY.—The Anniversary Meeting was held on Monday, November 30 (St. Andrew's Day). The following extracts from the address of the President (Sir William Thomson) are of considerable interest:—

"A Committee of the British Association, appointed for the purpose of reporting on the best means of comparing and reducing observations on terrestrial magnetism, has strongly recommended the re-establishment of a magnetic Observatory at the Cape of Good Hope. A conference on the subject was held between the Committee and Dr. Gill, the Astronomer-Royal of the Cape of Good Hope, last June, during his recent visit to England, which has resulted in an application to the Admiralty to carry this recommendation into practical effect in connection with the astronomical Observatory of the Cape of Good Hope (belonging to the Admiralty). This application is at present under the consideration of the Admiralty.

"A fundamental examination in astronomy, of great importance in respect to the primary observational work of astronomical Observatories, and of exceeding interest in connection with tidal, meteorological, and geological observations and speculations, has been definitely entered upon during the past year, and has already given substantial results of a most promising character. The International Geodetic Union, at its last meeting in the autumn of 1890, on the motion of Prof. Foerster, of Berlin, resolved to send an astronomical expedition to Honolulu, which is within 9° of the opposite meridian to Berlin (171° west from Berlin), for the purpose of making a twelve months' series of observations on latitude corresponding to twelve months' analogous observations to be made in the Royal Observatory, Berlin. Accordingly, Dr. Marcuse went from Berlin, and, along with Mr. Preston, sent by the Coast and Geodetic Survey Department of the United States, began making latitude observations in Honolulu about the beginning of June. In a letter from Prof Foerster, received a few weeks ago, he tells me that he has already received from Honolulu a first instalment of several hundred determinations of latitude, made during the first three months of the proposed year of observations; and that, in comparing these results with the corresponding results of the Berlin Observatory, he finds beyond doubt that in these three months the latitude increased in Berlin by one-third of a second, and decreased in Honolulu by almost exactly the same amount. Thus, we have decisive demonstration that motion, relatively to the earth, of the earth's instantaneous axis of rotation is the cause of variations of latitude which had been observed in Berlin, Greenwich, and other great Observatories, and which could not be wholly attributed to errors of observation. This, Prof. Foerster remarks, gives observational proof of a dynamical conclusion contained in my Presidential Address to Section A of the British Association at Glasgow, in 1876, to the effect that irregular movements of the earth's axis to the extent of half a second may be produced by the temporary changes of sea-level due to meteorological causes.

"It is proposed that four permanent stations for regular and continued observations of latitude, at places of approximately equal latitude, and on meridians approximately 90° apart, should be established under the auspices of the International Geodetic Union. The reason for this is that a change in the instantaneous axis of rotation in the direction perpendicular to the meridian of any one place would not alter its latitude, but would alter the latitude of a place 90° from it in longitude by an amount equal to the angular change of the position of the axis. Thus two stations in meridians differing by 90° would theoretically suffice, by observations of latitude, to determine the change in the position of the instantaneous axis; but differential results, such as those already obtained between Berlin and Honolulu, differing by approximately 180° in longitude, are necessary for eliminating errors of observation sufficiently to give satisfactory and useful results. It is to be hoped that England, and all other great nations in which science is cultivated, will co-operate with the International Geodetic Union in this important work."

Prof. Rücker received a Royal Medal, partly for his magnetic survey of the British Isles in co-operation with Prof. Thorpe.

"The high estimate that has been formed of the value of this magnetic survey is perhaps most easily appreciated from the very large sums that the Government Grant Committee have recommended should be contributed to aid in the completion of this work of international importance."

In responding for the medallists at the Annual Dinner in the evening, Prof. Rücker made a speech worthy of a medal in itself. An excellent report of it will be found in 'Nature' for 1891, Dec. 3, p. 112, whence we take the following extract:—

"There were sciences which, either in their origin or their development, were peculiarly our own. One of these was geology. Crowded up between our four seas was an epitome of the past history of the world such as he believed no other country possessed in an equally small area. Thus geologists were a natural product of our soil. But there was one particular in which he thought the President, more than most, would appreciate Prof. Lapworth's audacity and success. Though a Southerner, he had made a foray into Scotland, and had returned laden with spoil. It was true that he, too, had crossed the border, and he deeply regretted that he must confess that his track was marked by disturbances: but speaking for Prof. Lapworth and on his behalf—though without consulting him—he must admit that his offences were venial and that he was most to blame. He turned the most fundamental institution of Scotland,—its geological strata—upside down. There were only two ways of meeting an invader or an innovator such as this—with steel or with gold. They must confute him with the pen or reward him with a medal. The Council had chosen the better part of valour, and he was sure the Society did not question their discretion."

Comet Notes.—Comet Wolf may still be observed, but is getting rapidly fainter. Ephemeris for January:

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
Jan. 1....	4	14	28	14	11	Jan. 21....	4	21	33	11	33
5....		15	2	13	46	25....		24	9	10	55
9....		16	3	13	18	29....		27	4	10	15
13....		17	29	12	45	Feb. 2....	4	30	20	9	34
17....	4	19	21	12	11						

The brightness is 3·58 on Jan. 2nd, 1·65 on Feb. 3rd.

Berberich publishes in the Ast. Nach. the following Ephemeris of the Southern Comet discovered by Barnard on Oct. 2:—

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
Jan. 2....	15	44	55	41	48	Jan. 26....	16	14	46	34	23
6....		51	21	40	35	30....		17	48	33	6
10....		57	10	39	22	Feb. 3....		20	17	31	46
14....	16	2	24	38	8	7....		22	12	30	25
18....		7	5	36	54	11....		23	33	29	1
22....		11	12	35	39	15....	16	24	20	27	34

The brightness is 0·21 on Jan. 6th, 0·13 on Feb. 15. Both the above Ephemerides are for Berlin Midnight.

Comet Tempel-Swift will be extremely faint in January, but an ephemeris may still be useful for possessors of large instruments. The following is for Paris Midnight:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
Jan. 2....	3	20	21	27	14	Jan. 18....	4	30	24	25	27
6....		40	31	26	52	22....		44	10	25	0
10....		58	47	26	24	26....	4	56	54	24	34
14....	4	15	20	25	56						

The brightness is 1·1 on Jan. 4th, 0·3 on Jan. 28th. In all cases the brightness at discovery or rediscovery is taken as unity.

In view of the change which will take place in the superintendence of the 'Nautical Almanac' on the day of publication of this number, and the consequent changes in the annual volume which may follow, it is of interest to recall the additions made during the last few years by the retiring Superintendent, Dr. Hind, which are as follows:—

In the N. A. for 1892 an explanation is added (p. vii) of the method of determining the position of the Moon's Equator in calculating libration.

In the N. A. for 1893 a term is added to the nutation (p. v) of the form A cos $(\odot + B)$.

In the N. A. for 1894 the places of the standard and moon culminating stars are taken from the Greenwich Ten Year Catalogue (1880) instead of the Nine Year Catalogue (1872) as before. The terms of Nutation with argument $(☽ - \Gamma')$ have been included in the apparent places of polar stars, and the Day

Number E has been added. The mean longitude of the Moon is used instead of the true in computing the Nutation. Finally the places of Juno have been calculated from Mr. Downing's revised Elements.

In the N. A. for 1895 information is given as to the masses of the planets used in computing the perturbations of minor planets. Also Fabritius' method is used for calculating the apparent places of polars.

Many of these changes are apparently small, but may have involved considerable trouble in an office which must necessarily work with almost mechanical regularity.

A Plea for Astronomy in New South Wales.—Mr. John Tebbutt has issued a small pamphlet under this title, in which he contrasts the small amount of astronomical work done officially in the colony, with the large amount of meteorological information issued; which difference is not warranted by the money expended respectively on the two sciences.

The Newall Telescope.—In the early part of December the object-glass of this telescope was taken out in order to remove a little moisture that had found its way between the glasses. This opportunity was used to take the curves of the lenses and to examine as far as possible the surfaces by Foucault's method of testing. Want of length in the dark room prevented the testing of the shallow curve of the flint glass, but an examination of the deeper curve of this lens showed it to be a very fine surface entirely free from rings and very well polished. As far as the inspection of the crown lens could be made, this seems to be very fine, also as far as the working of the lens goes it seems to be a most admirable piece of work. In testing the deep curve of the flint lens at the centre of curvature it was supported in a wooden frame, which slightly nipped the lens across its horizontal diameter. In testing, the minute hole produced an image not round as it ought to have been, but quite like a line; this would have been quite fatal to good definition had it been due to the permanent shape of the glass. On relieving the pressure of the frame in which the lens was held the image *at once* resumed its proper shape; this shows very strongly the very great importance of freedom from constraint in the cell necessary to the best performance of an object-glass, and of course still more in the case of a mirror. A. A. C.

Large Meteors.—On November 22 last several fine meteors, probably Taurids, were observed at different places. People on the cliffs at Ramsgate saw what were believed to be two rockets fired by the North Sandhead lightship. The lifeboat, with Charles Fish as coxswain, at once put to sea. A heavy fog prevailed, and it was believed that a vessel had stranded on the Goodwin Sands. On returning at a late hour the coxswain reported that the lightship men had only observed two meteorites falling! On Dec. 20, at $8^h\ 38^m$, a meteor as brilliant as Venus was seen at Bristol. It first appeared in the head of Ursa Major, and fell almost vertically downwards from a radiant probably in the southern sky near ε Piscium.

OBSERVING POLE STARS IN THE VERTICAL OF POLARIS.—Prof. T. H. Safford proposes to determine the R.A.s of stars like δ Urs. Min. and Cephei 51 near 6^h or 18^h, whose transits occur at the maximum interval from that of Polaris, by observing when they cross the vertical of Polaris. For instance, the two stars 25 (H) Camelop. and Carrington 2965 will pass the vertical through Polaris at elongation within about half an hour.

THE TOTAL LUNAR ECLIPSE OF NOV. 15.—Observations were obtained at Bamberg (3 occultations, 2 measures of diameter), Berlin (4 occultations), Bonn (1 occultation), Breslau and Gern (shadows only), Lund (8 occultations), and Wien-Wahring. At most other stations the weather was too bad; and thus the material accumulated is scarcely worth the trouble of reduction.

MINOR PLANET 299 has been named Thora; 304, Olga; and 313 Chaldæa. Number 322 was discovered by Borelly at Marseilles on November 27.

AN interesting lecture was delivered by Mr. J. U. Poole to the Park Church Literary Society, Highbury, on "Navigation Past and Present." The lecturer said that he should take his illustrations of navigation in the past from the Scriptures, and accordingly commenced with Noah's Ark. He contended that its size and capacity had been systematically exaggerated by commentators, giving as his own estimate 10,000 tons, considerably less than half that of the 'Great Eastern.' He then dealt with other Scriptural navies and voyages, such as those of Solomon and St. Paul. In the rest of the lecture he dwelt on the two great epoch-making discoveries, those of the compass and the chronometer; and Mr. Poole was of course well able to deal with the latter.

Mr. F. C. COWELL has been elected Sheepshanks Exhibitioner at Trinity College, Cambridge. The Examiners were of opinion that the astronomical knowledge shown by the four candidates was very satisfactory, though they speak somewhat disparagingly of the mathematics.

DR. BERBERICH calls attention to a conjunction of Uranus with λ Virginis on 1892 January $2^d\ 13^h$ G.M.T.

WE congratulate Sir William Thomson on his elevation to the Peerage.

ERRATUM.—No. 182, p. 411 and list of contents, in title of Mr. Burnham's article, *for* β 6480 *read* β 648. The B.A.C. No. is correct.

Morgan & Kidd, *Greenwich.*

SIR GEORGE BIDDELL AIRY, K.C.B.

(A Photograph taken on his 90th Birthday).

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 185. FEBRUARY. 1892.

Within the last month English Astronomy has lost two very great names.

Two days after its commencement **Sir George Biddell Airy** died in his ninety-first year. His public life ended with his resignation of his office as Astronomer Royal ten years ago. Since then he had lived in perfect health at the White House, Greenwich, paying occasional visits to Playford, near Ipswich; and on one of these visits at the end of last year he suffered a severe fall, and subsequent internal complications necessitated a serious surgical operation. From these he appeared for a time to rally; but the shock to the system had been too great, and his strength gradually ebbed away. He died on Saturday afternoon, January 2, and was buried at Playford on January 7.

For some years **John Couch Adams** has been seriously ill. Partial recovery on several occasions has only been followed by relapses, and the last of these became so serious at the end of 1891 as to leave no hope of recovery. He died at the Cambridge Observatory early on Thursday morning, January 21, at the age of 72. He was buried at the Huntingdon Road Cemetery, close to the Observatory, on January 26, after a funeral service in the Chapel of Pembroke College, of which he was a Fellow at the time of his death. A service was also held in the chapel of St. John's College, of which he had been a Scholar, Fellow, and Tutor.

SIR GEORGE BIDDELL AIRY, K.C.B.

By the death of Sir George Airy we have lost, in the full ripeness of extreme old age, one of the most illustrious of English astronomers, whose scientific career has extended over nearly seventy years of a more than usually active life. When the account of the labours of this distinguished astronomer comes to be fully written, it must necessarily include almost a complete history of the progress of astronomy during the central half of the nineteenth century, and his name will always be remembered as one of the leading astronomers of Europe, who have, during that time, contributed to the perfecting of astronomical theories and to the advancement of practical astronomical observations. In the following notes, however, it is only possible to refer to the more important labours of Sir George Airy, for his separate contributions on various subjects exceed three hundred. These have embraced almost every section of astronomical and optical research, and may principally be found in the publications of the Royal, Royal Astronomical, and Cambridge Philosophical Societies.

George Biddell Airy was the son of Mr. William Airy, of Alnwick, Northumberland, where he was born on 1801, July 27. Most of his principal school life was, however, passed in the south of England, and for a time he resided with his uncle, Mr. Biddell, at Playford, near Ipswich. He received his preliminary education mostly at private schools at Hereford and Colchester. At an early age he gave indications of considerable mathematical talent, and through the interest of his friends, among whom was Mr. Ransome, the celebrated engineer at Ipswich, he was enabled to continue his studies, and to obtain the usual preliminary training necessary before entering the University of Cambridge. Having acquitted himself with great credit at school, he in 1819 entered as a sizar at Trinity College, and it need only be remarked that his college career was a very distinguished one. In 1822 he was elected a Foundation Scholar, and in 1823 he graduated as Bachelor of Arts, with the honours of Senior Wrangler and First Smith's Prizeman. He once remarked to the writer of this sketch, that he felt no anxiety whatever as to the position he would obtain on the Tripos list, as he felt convinced that he was certain to gain a high, if not the highest position; and, accordingly, during the month preceding his final examination, he relaxed his energies, and passed his time away, to a great extent, in recreative enjoyment. The result proved that he had not over-estimated his capabilities, for the number of marks credited to him was so greatly in excess of those of the Second Wrangler, that virtually he was without a competitor.

Such a brilliant academical career could not fail to produce its fruits. In 1824, Mr. Airy was elected a Fellow of Trinity College, and in 1826 he proceeded to the degree of Master of Arts. In the same year, when at the age of twenty-five, he was appointed to the Lucasian Chair of Mathematics, founded in 1663 by Mr. Henry Lucas, M.P. for the University, and rendered illustrious by its first occupants, Dr. Isaac Barrow and Sir Isaac Newton. This Professorship, although Mr. Airy's predecessors were very distinguished mathematicians, had become a mere sinecure; but he resolved to inaugurate a new system, and thus make it of real importance, so that it might become a practical benefit to the undergraduates. To give effect to this resolution, he gave notice soon after his appointment that it was his intention to give occasional public lectures on Experimental Philosophy. These lectures were continued during his residence at Cambridge, and many who subsequently obtained University honours have remembered with gratitude the clear and instructive explanations of the various subjects discussed. Among the most remarkable of these, the prelections on the Undulatory Theory of Light were especially of a high order. In one of these Prof. Airy called attention to an optical malady of the human eye, which is now known as "Astigmatism." He was himself a sufferer from this defect of vision, but he carefully examined its nature, and provided a remedy for it. The details of the many inquiries on which Prof. Airy at this time was engaged will be found in a valuable series of Memoirs in the 'Transactions' of the Cambridge Philosophical Society, of which he became a Fellow soon after taking his degree. For another very important paper communicated to the Royal Society, "On an Inequality of long period in the Motions of Venus and the Earth," Prof. Airy was, in 1833, awarded the Gold Medal of the Royal Astronomical Society.

In 1828, Prof. Airy resigned the Lucasian Professorship, having been appointed to succeed Prof. R. Woodhouse as Plumian Professor of Astronomy, and Superintendent of the newly-erected Cambridge Observatory. This Professorship was founded in 1704 by Dr. Plume, Vicar of Greenwich and Archdeacon of Rochester, for the promotion of practical astronomy, especially to describe the parts and uses of astronomical instruments, and to prove and exemplify the mathematical formulæ required in the reduction of observations. The appointment of Prof. Airy to this important post formed a striking epoch in modern astronomy; for no sooner had he taken up his residence at the Cambridge Observatory, when he resolved to commence a new and improved system of meridian observations and calculations which was eminently successful. This system was afterwards continued at Greenwich, and adopted in its main features at some of the foreign Observatories, to the great advantage of astronomical science.

To Prof. Airy's orderly and methodical mind, the usual habit of printing astronomical observations in the concise form hitherto adopted required a considerable reform, and he always expressed a strong opinion that astronomical observations were comparatively useless until they were completely reduced and published in a convenient form suitable for easy reference to the successive stages of calculation. He therefore set the example, which has since been generally followed, of thoroughly reducing his observations, thus rendering them fit for employment in the theoretical researches of the mathematical astronomer. How he succeeded may be seen in the first volume of the Cambridge Astronomical Observations, 1828–1835, in which the separate steps of the reductions are clearly defined, and also the results of the comparison between the observed and calculated places of the Sun, Moon, and planets, showing the error of the tabular place determined from each observation. Besides, it had been the general practice to neglect the observations of the superior planets except when they were near opposition, and even the Greenwich planetary observations of that period were few in number, and not sufficient to assist in any material degree for improving the theory. This improvement in the form of publication has been faithfully adhered to at most of the principal Observatories. This is a convincing proof of the practical talent employed in the original arrangement, which represented the observations and calculations from the initial readings at the telescope to the final calculated result. Prof. Airy was evidently not satisfied to follow in the usual routine which had hitherto been considered sufficient at most Observatories, and his natural love of work enabled him to succeed in creating a new epoch of meridian astronomy. At first he had no assistant at the Observatory, and the whole duties of the establishment entirely devolved upon himself. He was, however, young and vigorous, and so confident in his own resources, that, in addition to making the ordinary observations, he reduced them himself, prepared and copied out the results for the press, and finally examined the proof-sheets before publication. The practical acquaintance with the ordinary calculations of an Observatory obtained at this time was valued by him in after years, and he ever retained, throughout his official life, a sufficient knowledge of the successive steps of the various astronomical calculations to enable him to check and supervise any portion of the work requiring his consideration. In 1829, an assistant was appointed to the Cambridge Observatory, and subsequently a second assistant; but this permanent aid did not cause much relaxation to Prof. Airy, except that he was relieved for the most part of the daily routine observing with the instruments.

In 1832, an equatoreal by Jones of five feet focal length and 2¾ inches aperture, and an 8-feet mural-circle by Troughton and Simms, having an object-glass of 4½ inches aperture, were mounted under the superintendence of Prof. Airy. It is in connection with this mural-circle that the process of making a complete double observation of zenith distance, by direct vision and by reflexion, at the same transit of a star, was first employed. The Northumberland equatoreal, which was constructed from plans prepared by Prof. Airy, exhibits in an eminent degree the remarkable combination of mechanical skill and of optical science.

The same characteristics may also be observed in the more recent instruments erected at Greenwich. Constant and arduous as his official duties were at the Cambridge Observatory, Prof. Airy still found time to undertake many important scientific investigations. Of these we may refer to his well-known "Mathematical Tracts," on the lunar and planetary theories, the figure of the Earth, precession and nutation, the calculus of variations, and the undulatory theory of optics. This work was first published in 1826, a second edition in 1831, and subsequently a third and fourth editions. It still retains its great reputation, the object of the work being to promote in the University the study of physical, as distinguished from pure, mathematics. The article on "Gravitation," written for the 'Penny Cyclopædia,' and afterwards published in 1834 as a separate treatise, was also received with great favour. Prof. Airy proposed to himself to give a clear explanation of the perturbations of the solar system, as far as he was able, without introducing an algebraic symbol. His success has been much appreciated by the class of readers for whom the work was specially written. In 1832 he presented to the British Association a report on the recent progress of astronomy, which is still of great value.

During the last few years of the existence of the Board of Longitude, Prof. Airy was a member until the dissolution of the Board in 1829, after the death of its Secretary, Dr. Thomas Young. He was also an active member of the Sub-Committee appointed by the Astronomical Society of London, relative to the proposed improvements in the 'Nautical Almanac,' and many of the alterations incorporated in the new edition for 1834, published in 1833, were due to his suggestions. On the constitution of the new University of London, Prof. Airy was appointed a member of the Senate, but he held this appointment for a short period only.

In the summer of 1835 the important post of Astronomer Royal became vacant by the resignation of Mr. Pond. The Prime Minister, Lord Melbourne, on whom rested primarily the responsibility of recommending to the Crown some suitable person to fill the office, fixed upon the successful Director of the Cambridge Observatory, who immediately accepted the post of Astronomer Royal. It may be of interest to state here that though the Royal Observatory is placed under the official control of the Admiralty, the Astronomer Royal receives his appointment directly from the Prime Minister, and holds his warrant of office under the Royal Sign Manual. By this warrant he is directed "to apply himself with the most exact care and diligence to the rectifying the Tables of the motions of the heavens and the places of the fixed stars, in order to find out the so much desired longitude at sea, for the perfecting the art of navigation." These instructions are now a little out of date, for though they satisfy the great fundamental work of the Royal Observatory carried on since the days of Flamsteed, they necessarily make no allusion to the great physical departments which have gradually come into existence in recent years. It was a fortunate circumstance for astronomy that Lord Melbourne's choice fell on Prof. Airy, for, under his care, the scientific usefulness of the Royal Observatory was much increased, and the general progress of astronomy was materially encouraged by the prominent interest which he always took in the advancement of astronomical science. Under his superintendence, the Observatory was gradually augmented, old instruments of excellent construction, though still behind modern requirements, were laid aside, and new and complicated systems introduced which have raised the scientific standard of the observations.

Prof. Airy was appointed Astronomer Royal in September 1835, and it was not long before the effects of his new system of management became visible, both in the improved methods of observation and calculation. He, however, found the meridian instruments in a reasonably efficient state, especially Troughton's transit-instrument and mural-circle; both of which at that time had a very high reputation for ordinary meridian work, and fifteen years elapsed before they were replaced in 1851 with the more powerful transit-circle of the present day. But he was not long in perceiving that as the demand increased for accurate places of minute stars, so it would eventually become necessary to increase the optical power of the meridian instruments. This necessity became very apparent after the discovery in 1845 of a fifth minor planet. But one of the early distinguishing marks of Mr. Airy's superintendence was the great precision given to all the proceedings and discipline of the Observatory. He was very strict in all his relations with the assistants, and he expected his instructions, which were always given in writing, to be completely followed. But, above all, the assistants were required to make themselves acquainted with the daily prearranged order of work as sanctioned by him, and then to perform that allotted work to the best of their abilities, without the slightest deviation from his instructions. Mr. Airy also introduced at once the method of reducing and publishing the observations in the same detail as he was accustomed to do with so much advantage at Cambridge. One has now only to examine any volume of the 'Greenwich Observations,' published since 1836, to perceive that the successive stages in the reduction of an observation of a star or planet may be followed throughout, and they can thus be easily verified if necessary.

One of the first acts of Mr. Airy, after his appointment as Astronomer Royal, was to reorganize the internal arrangements of the Observatory with regard to the observing duties of the assistants. He proposed a new code of regulations according to a system which has continued in use, with only slight modifications, to the present time. So successful had these arrangements become that they served as examples to other Observatories, and M. Le Verrier was so impressed with the simplicity and efficiency of the system that, on becoming Director of the Paris Observatory in 1854, he adopted a weekly form of arrangement of observers precisely similar to that in use at the Royal Observatory. When Mr. Airy took up his residence at Greenwich, and during several years afterwards, the regular observations were chiefly confined to those made on the meridian with Troughton's transit-instrument, and the two mural-circles by Troughton and Jones. Following the traditions of the Observatory, Mr. Airy always considered that the meridian observations should have the first attention, and that it was the duty of the Astronomer Royal to carry out, most strictly, the terms contained in the original warrant of appointment. For this reason, when the progress of astronomy demanded the introduction of new subjects of observation, he took care that they should not interfere with the standard meridian work of the Observatory,—the accurate determination of the fundamental places of the Sun, Moon, planets, and principal stars. In one of his annual reports he states that "the observations of the Moon are the first object; next in reference to the wants of science, come the regular observations of planets, and below these, the observation of comets, &c., and the measures of double stars, &c. . . . Greenwich Observatory was built mainly for observation of the Moon, and of stars regarded as accessories to lunar observations; and at Greenwich Observatory alone can a continuous series of observations of the Moon be found."

Having been so strongly convinced of the uselessness of unreduced observations of the Moon and planets, Mr. Airy soon turned his attention to the huge mine of wealth locked up in the published volumes of the 'Greenwich Observations,' but totally unavailable in its printed state for immediate and future scientific use. To reduce all the observations of the Moon and planets made between 1750 and 1830 on one uniform plan was indeed an undertaking requiring much consideration. Though his time appeared to be fully occupied already, he brought the question of a proposed reduction of the Planetary observations before the General Committee of the British Association at the meeting at Cambridge in 1833. The Committee resolved to bring before the Government the national importance of making these reductions, and to request that the necessary funds may be provided for the payment of the stipends of a staff of computers, Mr. Airy undertaking to superintend the work gratuitously. A similar application was made in 1837 respecting the Lunar observations. Both applications were favourably received by the Treasury. The expense of carrying on these reductions, which were commenced at Cambridge in 1833, and finally completed in 1848, was thus provided for. The great bulk of the computations was performed at the Royal Observatory by a staff of computers independently of the ordinary assistants, and it was necessary to establish a separate department for the purpose. The results were published in considerable detail in three large quarto volumes, to which was added a supplementary volume extending the comparison of the lunar observations to 1835. The magnitude of this work is shown by the large number of manuscript folio volumes of calculations still preserved in the archives of the Royal Observatory. They form an appropriate monument of the genius and perseverance of the distinguished originator of a work which has furnished the fundamental data of the sublime mathematical investigations of Hansen, Le Verrier, and others. In the words of Sir John Herschel, we may state that this laborious work "has rendered uniformly available to the theorist no less than 8000 observed places of the Moon, compared with theory by the computation of the same number of places, each separately and independently calculated from Plana's formulæ; points which I mention as most effectually tending to convey to the mind of anyone who has ever calculated a place of the Moon *ab initio*, some adequate idea of the immensity of the undertaking." The Moon's tabular place for mean time of transit was computed by Damoiseau's 'Tables de la Lune' (1824), modified by the introduction of Plana's coefficients and new terms. These corrections added to the personal labour of Mr. Airy, as it was necessary to compute several new tables, which were combined with Damoiseau's printed tables when the arguments permitted such combination. The Gold Medal of the Royal Astronomical Society was awarded to Mr. Airy for the Planetary reductions, and a Testimonial (equivalent to a gold medal) for the Lunar reductions.

In the beginning of 1836 Mr. Airy proposed to the Board of Visitors a scheme for the establishment of a Magnetical and Meteorological Observatory, and the sanction of the Government was obtained in 1837 for the erection of a building in which, during 1838 to 1840, occasional observations were made. In the summer of 1840 the Royal Society proposed to recommend to the Treasury the importance of making a series of observations at some place near London, in general correspondence with Captain J. Clark Ross's surveying expedition to the Antarctic Ocean, in conformity with the plan drawn up by Prof. Gauss, of Göttingen, for the simultaneous observation of the movements of the magnetic needle at various selected stations in both hemispheres. Mr. Airy lost no time in proposing to the Council of the Royal Society the selection of the Royal Observatory for this purpose, as facilities already existed there for making the necessary observations. The Astronomer Royal's proposal was immediately laid before the Lords of the Treasury, and his complete plan of observations was sanctioned by them. At the end of October 1840 a small staff of assistants was engaged, and a system of regular two-hourly observations commenced. At first it was intended to carry on this new department of the Observatory for a limited period only, and the Government plainly intimated that the period would be strictly defined by the length of time occupied by the Antarctic expedition. Mr. Airy was, however, so impressed with the importance of continuing the observations that, at his urgent suggestion, the establishment was continued as a permanent department of the Observatory, with certain modifications, both personal and administrative. The method of two-hourly observations, which was always a harassing work for the observers, was superseded in 1848 by a system of photographic registration by an apparatus invented by Mr. Charles Brooke, F.R.S. Several valuable researches founded on the magnetical observations were contributed by Mr. Airy to the Royal Society, and published in the 'Philosophical Transactions.'

After the destruction of the standards of length and weight in the great fire at the Houses of Parliament in October 1834, a Commission was appointed to consider the general question of Standards, of which Mr. Airy was appointed the Chairman. He took a considerable personal interest in the detailed work of the Commission, and prepared the report presented to Parliament in 1841. A Committee was afterwards appointed to superintend the construction of new parliamentary standards. Mr. Airy again acted as Chairman, but the general superintendence of the new standard of length was intrusted to Mr. Francis Baily, and after his death in 1844 to the Rev. R. Sheepshanks, and the standard of weight to Prof. W. H. Miller. An elaborate and valuable exposition of the methods employed in these delicate experiments is given by Mr. Airy in his "Account of the Construction of the New National Standard of Length, and of its principal copies," contained in the 'Philosophical Transactions' for 1857.

In 1838 Mr. Airy's attention was occupied by some important experiments with the object of ascertaining the amount of disturbance produced on the compass-needles in iron-built ships, and from the results of the experiments, carried on principally in H.M.S. 'Rainbow,' in Deptford Dockyard, and the theory developed from them, he devised a practical method of mechanical correction of the deviation of the needle by means of magnets and iron, which was at once adopted in all iron ships. Although some improvements in the method of correction have been made, the general principle as devised by Mr. Airy is still in use. His discussion of the observed deviations of the compass is given in two papers presented to the Royal Society, the first in 1839, and the second in 1855.

Mr. Airy was requested by the Government to draw up a code of instructions to the officers employed in the determination of the boundary line between Canada and the United States. This was done by him with great exactness, and to insure success the officers were located in 1843 for several weeks at the Royal Observatory, for the purpose of receiving the special instruction which Mr. Airy considered adapted to the Service. As the result of that careful instruction, the officers were enabled to overcome difficulties described as surpassing the conception of any European. The most difficult part of the boundary was the drawing of a straight line of nearly seventy miles in length to join two defined points passing through impervious forests, steep ravines, and dismal swamps. As it appeared almost impossible to effect this by ordinary survey, Mr. Airy recommended that the difficulty might be overcome by computing the azimuths of the two ends from the observed latitudes and differences of longitudes, and then the two parties should cut through the woods in the assigned directions, one from each terminal station. The two parties thus cutting independently of each other, drew lines which met within 340 feet, or equivalent to a quarter of a second of time in the difference of longitudes, which was determined by the transmission of chronometers by a very circuitous route from one extremity of the line to the other. The complete success of this important international work was due, in a great measure, to the foresight and advice of the Astronomer Royal. Mr. Airy also gave similar assistance preparatory to the definition of the Oregon boundary.

Among other miscellaneous matters on which Mr. Airy was engaged at this time, was the reduction of an enormous mass of Tidal observations made on the coast of Ireland, according to instructions drawn up by himself, in conjunction with Colonel Colby, Superintendent of the Trigonometrical Survey in Ireland. The results obtained from these reductions led to some conclusions of great interest, and they formed the subject of a paper contributed by Mr. Airy to the Royal Society. He also undertook the care of the printing of the account of Mr. (afterwards Sir Thomas) Maclear's observations of the verification and extension of La Caille's Arc of Meridian; and he assisted Prof. W. Struve in his determination of the longitude of Altona, in connection with the operations for determining the longitude of Pulkowa. This determination of longitude between Greenwich and Altona was made in the summer of 1844, by the transmission, to and fro, of several chronometers.

A very important operation of this kind was also undertaken by Mr. Airy in the summer of 1844, in connection with the determination of the great arc of parallel, extending from the Oural river in Eastern Europe to the island of Valencia, Ireland. Prof. Struve carried out this great work as far west as Greenwich, and Mr. Airy agreed to complete the arc by an accurate determination of the difference of longitude between it and Valencia. This operation was performed by the transmission of thirty pocket chronometers backwards and forwards, eight times each way between Greenwich and a temporary observatory at Kingstown, and ten times each way between Kingstown and a temporary observatory on the hill Geokaun, near the centre of the island of Valencia. The numerous observations and chronometer comparisons were exhaustively discussed by Mr. Airy, and the resulting difference of longitude between the Royal Observatory and Valencia was obtained with great exactness. This accuracy was afterwards confirmed by two independent determinations, made in 1862 and 1866, by the transmission of galvanic signals from one station to the other, as may be seen by the three separate resulting differences of longitude for the station of the Atlantic telegraph cable at Foilhommerum Bay, Valencia, determined from the observations made in 1844, 1862, and 1866,—$41^{m}\ 33^{s}\cdot24$, $41^{m}\ 33^{s}\cdot38$, and $41^{m}\ 33^{s}\cdot29$ respectively. Since the introduction of railway time-signals, Mr. Airy took the earliest opportunities, in cooperation with the Directors of the Observatories of Cambridge, Edinburgh, Brussels, and Paris, to determine the differences of longitude between each of these Observatories by the galvanic method. These operations were successfully completed in 1853 and 1854.

In 1845 Mr. Airy was appointed one of the three members of a Royal Commission to test the separate merits of the different railway-gauges then in use. The frequent sittings occupied a considerable time, and an enormous amount of professional evidence was given. Mr. Airy took a great interest in the work of this Commission, and he, in company with Prof. Barlow, made in January 1846 several independent experiments during a series of trial-trips of extraordinary velocity. The report of the Commission showed much skill in its compilation. It embraced the whole bearings of the question; the difficulties of break of gauge were duly considered; and, finally, after acknowledging that in some respects the broad gauge was to be preferred, the Commissioners recommended that, as the majority of railways had adopted the narrow gauge, it alone should be maintained and permitted in all railways then under construction, or hereafter to be constructed in Great Britain. The Great Western Railway having adopted the broad guage was exempted, but in 1892 it also is to be transferred into a narrow gauge line throughout its whole system, for the convenience of the interchange of traffic. Mr. Airy also served on another important Commission for inquiring into the best method of construction of the Metropolitan Sewers.

In the spring of 1846 Mr. Airy made some very interesting experiments near Kensal Green, for the purpose of ascertaining the amount of vibration caused by the passing of railway trains through tunnels on the London and North-Western Railway, in connection with the possible effect of a proposed extension of the Greenwich Railway, through a tunnel in Greenwich Park, upon the instruments at the Royal Observatory.

During the total eclipse of the Sun of 1842, July 8, Mr. Airy and Mr. Francis Baily were enabled to view with distinctness the rose-coloured protuberances and other phenomena, the separate accounts of which were read at the following November meeting of the Royal Astronomical Society. Mr. Airy's observations were made on the Superga hill, about five miles from Turin, while those of Mr. Baily were made at Pavia. These eclipse observations created a remarkable interest, as being among the first which revealed, with reliable authority, the actual form and magnitude of the prominences, the origin of which has since formed, during succeeding eclipses, one of the principal objects of scrutiny. Mr. Airy naturally took more than usual interest in the solar eclipse of 1851, July 28, when he organized an expedition to Sweden and Norway, consisting of himself and two assistants. He observed the eclipse favourably at Gottenburg, and the assistants at Christiania and Christianstadt. Mr. Airy had the good fortune to witness a third eclipse in that of 1860, July 18. On this occasion he collected together a large party of observers, who were conveyed to Bilbao and Santander in H.M.S. 'Himalaya.' This well-known troopship was specially selected by the Admiralty for the use of the expedition. The total shadow-path in this celebrated eclipse passed over the northern part of Spain. Mr. Airy selected for his station a hill near Hereña, and was favoured with a clear sky during the totality. In this expedition, and also in that of 1842, he was accompanied by Mrs. Airy, who acted as his assistant by observing the general appearance of the eclipse to the naked eye.

In 1843 Mr. Airy represented to the Board of Visitors the scientific importance of adding to the instruments of the Royal Observatory a new altazimuth to be used expressly for the observation of the Moon. By an instrument of this construction he considered that observations of the Moon may be obtained in any part of the sky, especially when near conjunction. The subject had for many years previously occupied the serious attention of Mr. Airy, and he often expressed an opinion that means should be provided for rendering the series of observations of the Moon more complete than can be made by the methods usually recognized in our observatories. Mr. Airy saw clearly that there was but one instrument with which observations could certainly be made possessing the required accuracy. He explained to the Board the details of his plan of the mounting of a new altitude and azimuth instrument, to be constructed with great massiveness and strength, with a similar exclusion of adjusting power which he had previously adopted in the zenith sector designed by him for the Ordnance Survey. The altazimuth, the heavy portions of which were made by Ransomes and May, of Ipswich, was completely mounted and ready for use in May 1847. Mr. Airy had great confidence in this instrument, as he considered it of the firmest and best mechanical construction, in which all the parts subservient to measures had been examined by himself or his assistants; and he felt no doubt of accomplishing the difficult object to which he had addressed himself, of making observations east or west of the meridian with nearly the same accuracy as observations made on the meridian.

The altazimuth was the first of the massive instruments constructed from the plans of Mr. Airy. It was soon followed by the transit-circle, which superseded Troughton's transit-instrument and mural-circle at the beginning of 1851. This new instrument was also constructed by Ransomes and May as engineers, and by Mr. Simms as optician, both of whom carried out the designs of the Astronomer Royal with scrupulous attention. As an illustration of the strict demands of Mr. Airy, it may be interesting to repeat here an anecdote related by Mr. George Biddell, who personally superintended the mounting of the transit-circle, as reported in the 'Observatory' for August 1891:—"Mr. Biddell described to a small knot of interested guests the dismay of the workmen and their employers at the demands of Sir G. B. Airy, especially those relating to the pivots. These were to be of chilled iron, six inches in diameter, and perfect cylinders to within $\frac{1}{30,000}$ inch! No error of this magnitude was to be discernible with a delicate spirit-level; and after trying all the most delicate methods of turning then known, the requisite accuracy was obtained by sheer labour—rubbing down bit by bit all the places which this same spirit-level indicated as too high. Each of the pivots cost six weeks of such labour!"

The reflex zenith tube was erected in 1851, and was designed by Mr. Airy to supersede the old 25-feet zenith sector, mounted by Troughton in 1833 for the purpose of making observations of γ Draconis and other stars that pass the meridian near the zenith of Greenwich.

Soon after the establishment of the telegraph system in England, Mr. Airy considered that it would add to the national usefulness of the Royal Observatory if its application could be introduced as a means for an accurate dissemination of Greenwich mean time throughout the country, and also for the chronographic registration of transits observed with the transit-circle, altazimuth, and equatoreals. With the cordial cooperation of the various Telegraphic Companies, especially of Mr. C. V. Walker, F.R.S., Telegraphic Engineer to the South-Eastern Railway, arrangements were made in 1852 for connecting the Observatory with the general telegraphic system; and soon after the distribution of the Greenwich time-signals was commenced.

The method of registering the transit observations on a chronograph was adopted in 1854, after many experiments, and it became the standard method of recording transits. At first Mr. Airy was not favourable to the plan of registration in the form adopted by American astronomers, but after some practical modifications of his own he was convinced of its superiority over the eye-and-ear method—the probable error of a complete transit over the whole system of wires being reduced from $0^s{\cdot}028$ to $0^s{\cdot}017$. Mr. Airy, however, believed that there were some inconveniences attached to the chronographic work, as much time was consumed in the galvanic preparations, the barrel-work, and the translation of the puncture indications into figures. But in his annual report for 1854 he remarks:—"But among the observers who use it there is but one opinion on its astronomical merits—that, in freedom from personal equation and in general accuracy, it is very far superior to the observation by eye and ear."

The extensive series of pendulum experiments made in the Harton Colliery, near South Shields, during the autumn of 1854, was one of the most elaborate undertakings of Mr. Airy, extraneous to the Observatory, and required his close attention for some time previously. So perfect were the arrangements made by him that no break of any kind occurred in the observations from the beginning to the end of the experiments. Mr. Airy always took a great interest in pendulum observations as a means of ascertaining the variation of gravity on descending from the surface to the bottom of a deep mine. In 1826 he and Dr. Whewell attempted to solve this important question by making a series of pendulum experiments in Dolcoath mine, one of the deepest mines in Cornwall. This first attempt of Mr. Airy resulted in failure, owing to an accident to a portion of the apparatus, which mysteriously caught fire in transit from the bottom of the shaft to the surface. In 1828 a second attempt was made by the same observers, with the addition of the Rev. R. Sheepshanks and two assistants. On this occasion the principal difficulties that troubled the observers two years before were got over, but others unforeseen occurred, the most important of which presented itself in the irregular and varying form of the pendulum's knife-edges. After tedious but apparently successful experiments on these, the observations were suddenly stopped by what is called a fall in the mine, which, though it amounted to only a few inches, was sufficient to interrupt the working of the pumps. The result of this was that the lower station was flooded by the rising of the water, and for the second time Mr. Airy's attempt to solve this question was defeated. So far as he was concerned, the problem remained in abeyance until the latter part of the summer of 1854, when, finding that several circumstances occurred favourable for a renewal of the experiment in one of the deepest coal-pits near South Shields, he resolved to make another attempt, with the aid of six skilled astronomical assistants, two of whom were from the Royal Observatory, and one each from the Observatories of Oxford, Cambridge, Durham, and Red Hill. The observations were made during the absence of Mr. Airy, strictly according to his instructions, and occupied just three weeks. They consisted principally in accurately noting, simultaneously at the upper and lower stations, the vibrations of an invariable free pendulum, in comparison with the vibrations of a clock-pendulum placed immediately behind the free pendulum, at the moment when the two pendulums swung in coincidence. The operations of every kind were performed with the greatest regularity and accuracy; and Mr. Airy remarked, at the close of the experiments, that "perhaps it is not too much to say that no work of observation requiring the cooperation of numerous observers, has ever been effected in a more systematic manner." The instruments were inspected by Mr. Airy before and after the experiments were made. In the meantime the management was intrusted to the Superintendent of the party, between whom and Mr. Airy a continuous correspondence was carried on during the progress of the operations. The immediate result derived from these interesting experiments was that, supposing a clock were adjusted to go true time at the top of the mine, it would gain 2¼ seconds per day at the bottom—or, in other words, gravity is greater at the bottom of the mine than at the top by $\frac{1}{19,190}$ part.

In the autumn of 1855 Mr. Airy addressed a circular to the individual members of the Board of Visitors, pointing out that the Sheepshanks equatoreal, erected by him in 1838, though a valuable addition to the instruments of the Observatory, was subject to defects of construction, and quite inadequate for the general purposes of modern astronomy. He had for some time previously considered that means ought to be found for providing a telescope of increased aperture, capable of being employed on the highest class of extra-meridian observations, and mounted upon a frame of great stability in a better position. Mr. Airy, however, took the precaution of stating to the members, while recommending such an instrument, that he had no intention of interfering with the steady adherence to the meridian system, which both reason and tradition had engrafted on the Observatory. The members of the Board were enthusiastically in favour of Mr. Airy's proposition. He had previously ascertained from Merz and Son, of Munich, that they had on hand an object-glass of 13 inches aperture, and 17¼ French feet focal length, the price of which was about £1100. The assent of the Admiralty was obtained, and this object-glass was placed in the hands of Mr. Airy in the summer of 1857. He made a careful examination of its defining power by making various trials on test-objects, and was well satisfied with its performance. In the meantime the construction of the whole of the heavy mounting was intrusted to Messrs. Ransomes and Sims, and the optical parts to Mr. Simms, who, as with former instruments, worked from plans designed or sanctioned by Mr. Airy. This excellent equatoreal was brought into use in the spring of 1860.

So early as 1857 Mr. Airy gave an oral statement at a meeting of the Royal Astronomical Society on the means which would be available for correcting the measure of the Sun's distance in the following twenty-five years, a problem which he considered to be one of the noblest in astronomy. In this statement Mr. Airy reviewed the applicability of the methods of Halley and Delisle to the transits of Venus in 1874 and 1882; and he pointed out the most favourable stations at which the observations could be made. At the same time he drew the attention of astronomers to the near approach of Mars in 1860 and 1862, and to the importance of observing this planet and neighbouring stars as an excellent independent method of determining the solar parallax, according to a plan which he proposed. The preparations for a British expedition to observe the transit of Venus in 1874 were undertaken by Sir George Airy, and he had the complete personal control of the enterprise. Altogether twenty-three telescopes were collected by him, nine of which were provided with double-image micrometers, and five photo-heliographs. Nine portable transit-instruments and six altazimuths were supplied for the determination of local time. All the preparatory work at the Observatory, and the practical training of the observers, entailed an immense amount of thought on Sir George Airy. Only those who were engaged in the enterprise can have any adequate conception of the requirements of so extensive an expedition. He also undertook the superintendence of the reductions, in which he was most ably assisted by Lieut.-Col. Tupman, to whom was intrusted the general charge of the work. The calculations occupied some years, but they were sufficiently advanced in 1877 to allow a preliminary report to be made to Parliament. The complete volume, containing the details of the observations and reductions, was issued in 1881, the preface being signed by Sir George Airy on June 6, only about two months previously to his retirement.

In 1873 Sir George Airy commenced the formation of a new department of the Royal Observatory, with the object of continuing the valuable series of solar observations hitherto carried on with Mr. De la Rue's photo-heliograph at Kew. This instrument was transferred to Greenwich, and used until the return of the Transit-of-Venus expedition, when one of the photo-heliographs belonging to the Observatory was substituted. Thus the continuity, so necessary in such records, is preserved from the earliest observations to the present time. In the same year a spectroscope was constructed and attached to the great equatoreal, as it was decided to include solar spectroscopy in the regular course of observations, as well as the occasional examination of the spectra of stars and nebulæ. In the establishment of this new departure in physical astronomy at the Observatory, Sir George Airy was ably seconded by Mr. Christie, the present Astronomer Royal.

The testing and improvement of marine chronometers was a subject which occupied the attention of Sir George Airy during the whole of his official life. At first he had a strong opinion that the time occupied in the management of the commercial part of the business relating to chronometers was most injurious to the astronomical efficiency and general reputation of the Observatory. Some of the chief inconveniences that interfered with his personal attention to astronomical work having been removed, he did all in his power to encourage the makers to improve the mechanical construction of chronometers. He took a special interest in some improvements proposed by Eiffe and Molyneux for the correction of chronometers at extreme temperatures. An account of these was printed under his superintendence, and it led to the most beneficial results in the construction of these important instruments. Many of the more recent delicate appliances for the improvement of chronometers are the result of Sir George Airy's suggestions to the makers. He also gave important advice to the makers in every stage of the manufacture of the Great Westminster clock and bells at the Houses of Parliament, and was consulted by the Government in many ways on scientific questions.

His assistance in carrying out the Act applying to measures for the sale of gas, and his practical suggestions to the Royal Commission for the examination of lighthouses, were much appreciated.

Sir George Airy had a strong opinion during most of the time since he became Astronomer Royal that an explanation of the Lunar Theory, in which every coefficient is expressed by simple numbers from the beginning to the end of the process, would be very desirable. His ideas on the subject having by degrees assumed an orderly form, he exhibited the leading points of such a theory at the Meeting of the Royal Astronomical Society in January 1874. Though really too late in life, he did not shrink from undertaking so colossal a work; at any rate, he considered that, if he were unable to complete the research, he might leave the stages of calculation in such a state that a successor could continue it. The computations were so arranged that, with a little instruction, computers were employed in the preparation of the principal numerical details of the work, every numerical coefficient being accompanied with a symbolical correction whose value will sometimes depend on the time, but in every case is ultimately to be obtained in a numerical form. The progress of the calculations was often delayed by the pressure of Sir George Airy's other occupations, not only by the ordinary official work of the Observatory, but also by the completion of the preparations, reports, and calculations for the transit of Venus in 1874, besides other extraneous scientific calls upon his time. Sir George Airy fully expected to see the completion of his labours before his retirement from the Observatory, but it was not until August 1886 that the work finally left his hands for the press. After all this personal labour bestowed on the calculations, he found that, owing to some unaccountable error in them, the results were not so satisfactory as he wished. One of the principles of his operations was to arrange the fundamental mechanical equations in a form suitable for the investigation, and then to substitute in the terms of these equations the numerical values furnished by Delaunay, and to examine whether the equations are satisfied. Sir George was much disappointed with the result, for he remarks :—"With painful alarm, I find that they are not satisfied; and that the discordance, or failure of satisfying the equations, is large." It is, however, pleasing to note the energy of the aged astronomer, who at once endeavoured to find out the cause of the discrepancy, which he attributed to having unfortunately assumed the existence of a solar term, with a coefficient which he had called M, and which he supposed to exercise an influence on the lunar motions. In this he was mistaken. Though Sir George proceeded to rectify this error, he eventually found that the labour involved in the substitution in several places of new piles of figures for those which he had furnished, was far greater than he could bear, and he was reluctantly compelled to close the subject, with the remark, "I keep up my attention to the general subject, but with my advanced age (eighty-eight) and failing strength, I can scarcely hope to bring it to a satisfactory conclusion."

The miscellaneous scientific researches of Sir George Airy have been so numerous, that it is impossible to give here an abstract of, or even to enumerate the titles, of the greater number. For these the reader should refer to his contributions to the Royal Society, the Royal Astronomical Society, the Cambridge Philosophical Society, and the Reports of the British Association. He was also an occasional contributor to the 'Philosophical Magazine,' the 'Athenæum,' usually above the signature of A. B. G., and to other scientific works. A memoir on the use of silvered glasses for the mirrors of reflecting telescopes was read before the Cambridge Philosophical Society in 1822, before he had taken his degree. This was followed by more than twenty papers, most of which are on optical subjects. Among his contributions to the Royal Society the following titles of memoirs, a few of which have been previously referred to, may be taken as examples:—"On an inequality of long period in the motions of the Earth and Venus;" "Experiments for the correction of the deviations of the compass in iron-built ships;" "On the laws of the tides on the coasts of Ireland and some other places;" "On the eclipses of Agathocles, Thales, and Xerxes;" "Account of pendulum experiments undertaken in the Harton colliery to determine the mean density of the Earth;" "Account of the construction of the new national standards of length;" "On the strains in the interior of beams;" and "On the diurnal inequalities of terrestrial magnetism." Most of Sir George Airy's astronomical papers are inserted in the 'Memoirs' and 'Monthly Notices' of the Royal Astronomical Society, and they amount to more than 200. The following titles represent a few of the more important:—"On the Mass of Jupiter;" "Observations of the Total Solar Eclipses of 1842 and 1851;" "On the time and rotation of Jupiter;" "On the determination of the Orbits of Comets from observation;" "Correction of the elements of the Moon's orbit, deduced from the Greenwich observations;" "On the eclipses of Agathocles, Thales, and Sticklastad;" "On the movement of the Solar System in Space;" "On the apparent projection of Stars upon the Moon's disc in occultations;" "Account of some circumstances historically connected with the discovery of the Planet exterior to Uranus;" "On the difference of Longitude between the Observatories of Brussels and Greenwich, as determined by galvanic signals;" and "Determination of the longitude of Valencia in Ireland by galvanic signals in the summer of 1862."

Besides the researches mentioned above, Sir George Airy was the author of the articles on Trigonometry, Tides and Waves, and the Figure of the Earth, in the 'Encyclopœdia Metropolitana,' as well as separate volumes on the algebraical and numerical theory of observations, and the combination of observations; an elementary treatise on Partial Differential Equations, and treatises on Sound and Magnetism. His useful and most popular work, founded on his six lectures on astronomy delivered at the Museum at Ipswich in 1848, has gone through several editions. Among many papers on other subjects, Sir George has published his ideas relating to the locality of the invasion of Britain by Julius Cæsar, and questions of this nature always afforded him considerable pleasure, and he freely gave his opinion on various historical and archæological points of interest in a series of tracts collected into a volume for private circulation. He also issued a critical work entitled 'Notes on the Hebrew Scriptures.'

In addition to the Ipswich lectures, Sir George Airy delivered a popular lecture on solar eclipses at the Meeting of the British Association at Manchester in 1861; three lectures at the Royal Naval School of Architecture, South Kensington, on magnetical errors, compensations, and corrections, with special reference to iron ships and their compasses, in March 1865; and a series of lectures on magnetism, at Cambridge, during Easter term, 1869. In October 1854, on the conclusion of the pendulum experiments in the Harton Colliery, he delivered an explanatory lecture to a large audience in the Central Hall, South Shields, including several hundreds of working-men, members of the Mechanics' Institution and Working Men's Institution, who were present at the express invitation of the Astronomer Royal. A verbatim report of this lecture has been published, giving a detailed account of the experiment.

As editor of the works of other astronomers, Sir George superintended the printing of Groombridge's 'Catalogue of Circumpolar Stars;' the observations made by the Rev. Fearon Fallows at the Cape; and the observations of eclipses of Jupiter's satellites, occultations of the stars by the Moon, and other astronomical phenomena, made by the Rev. Thomas Catton, at Cambridge; in addition to Maclear's arc of meridian, already mentioned.

The foregoing notes refer generally to Sir George Airy's personal investigations, many of which were extraneous to the ordinary work of the Astronomer Royal. It is known to all astronomers that, under his management, the reputation of the Royal Observatory was jealously guarded, especially in relation to the standard meridian observations of the Sun, Moon, planets, and principal stars. Though, however, no one more than Sir George respected the traditional objects for which the Observatory was founded, yet he was ever ready to add new departments of inquiry, as soon as the advancing state of science demanded them. He found in 1835 that the observations were almost solely confined to the meridian, but, as Astronomer Royal, he added to these the magnetical and meteorological, the altazimuth, and the physical departments, all of which have been continued as part of the daily routine work of the Observatory. New instruments of increased stability and power were substituted for those in use by Mr. Pond. The general organization of the Observatory, so as to satisfy the modern requirements of astronomy, was always a subject to which Sir George Airy paid much attention, and he spared no pains to make it as perfect as possible. The magnitude of the operations carried on under his supervision can hardly be realized except by a reference to the different sections of the volumes of the Greenwich observations or to the annual reports which he was accustomed to read at the Meetings of the Board of Visitors. These reports, from 1836 to 1881, form a complete history of the proceedings of the Observatory during the presidency of Sir George Airy. This system of making an annual printed report on the state of the Observatory has been adopted with advantage by other institutions, and very few of the Directors of the principal Observatories have failed to follow the custom of the Astronomer Royal. Though Sir George Airy had attained his eightieth year before his retirement on 1881 August 15, his official vigour remained to the end, and nothing would prevent him from occupying his accustomed seat until noon of that day, when he, with some emotion, formally handed over the keys to his successor.

Sir George Airy was elected a Fellow of the Royal Society on 1836 January 21. He had a seat on the Council on several occasions, and during the two years, 1871–1873, he filled the distinguished post of President. For some years past he has been the father of the Royal Astronomical Society, having been elected a Fellow so far back as 1828 May 9. He was placed on the Council in 1830, and remained a member continuously until 1886. During that long interval he was elected President on five occasions—in 1835–1837, in 1844, on the death of Mr. Baily, until the Annual Meeting; in 1849–1851, 1853–1855, and 1863–1864. It became his duty to deliver the Presidential Addresses on presenting the Gold Medal of the Society to Sir John Herschel, Prof. Rosenberger, Captain W. H.Smyth, M. Otto von Struve, Dr. De Gasparis, Mr. Charles Rümker, and the Rev. W. R. Dawes. He was the President of the British Association at the Meeting held at Ipswich in 1851. Sir George Airy was a Fellow of the Cambridge Philosophical Society; one of the eight Foreign Members of the French Institute; an Honorary Member of the Institution of Civil Engineers, of the Royal Society of Edinburgh, and of the Royal Irish Academy; and a Corresponding Member of several foreign Academies. In 1831 Sir George received the Copley Medal of the Royal Society for his papers on optical subjects, and in 1845 the Royal Medal for his paper on the laws of the tides on the coasts of Ireland. He has twice been awarded the Gold Medal of the Royal Astronomical Society and a testimonial in 1848 for his reduction of the Greenwich lunar observations from 1750 to 1830, a reference to which has already been made. The Institution of Civil Engineers awarded him a Medal for his suggestions on the construction of bridges of very wide span. He has also received the Lalande medal of the French Académie des Sciences, and the Albert Medal of the Society of Arts. In 1875 Sir George

Airy was presented with the honorary freedom of the City of London, enclosed in an elegant gold casket. He was a D.C.L. of Oxford, an LL.D. of Cambridge and Edinburgh, and an Honorary Fellow of Trinity College, Cambridge.

On 1871, May 17, the great scientific services of Mr. Airy were officially recognized by the Government by their conferring on him a Civil Companionship of the Bath. On July 30 in the following year he was raised to the higher dignity of a Knight Commander, and shortly afterwards was knighted by Her Majesty the Queen at Osborne.

The great scientific reputation of Sir George Airy was acknowledged by several foreign Governments, and many honorary titles were conferred upon him. Besides those received from his own Sovereign, he had the honour of being a Chevalier of the Order *Pour le Mérite* of Prussia; of the Legion of Honour of France; of the Polar Star of Sweden; of the Dannebrog of Russia; of the Rose of Brazil, &c., as well as being the recipient of many personal favours, not the least of which was a present from the Emperor of Russia of a gold box set with diamonds. But though Sir George was so much honoured, he was really a man of essentially simple nature and habits, and cared little for the social advantages which many would consider naturally belonged to one in his high scientific position.

Sir George Airy married on March 30, 1830, Richarda, daughter of the Rev. Richard Smith, of Edensor, Co. Derby. Lady Airy died on August 13, 1875, a few days after the celebration of the bicentenary of the foundation of the Royal Observatory. After Sir George's retirement in 1881 he took up his residence at the White House, pleasantly situated on the Greenwich side of Blackheath, and overlooking the Park and the Observatory. Here he continued to reside in general good health of mind and body, taking his usual daily constitutional walk and occasional visits to his country-house at Playford. Many will remember his hale appearance on the day when his family and friends assembled in honour of his ninetieth birthday, when he received in person the congratulations of a large and distinguished company. His last public act was peculiarly interesting. On the actual ninetieth anniversary of his birthday he gracefully performed the ceremony of turning on, at 9 P.M., for the first time, the gas intended to illuminate the clock of the parish church of St. Alphege, Greenwich. As Astronomer Royal he was officially concerned in spreading a knowledge of accurate Greenwich time throughout Great Britain; and the excellent speech which he delivered on this occasion showed that, in his extreme old age, it was still a pleasure to him to assist in a ceremonial having for its object the exhibition of Greenwich time to his fellow-townsmen throughout the twenty-four hours.

Some time before his illness Sir George had an accidental fall at his house at Playford. It was afterwards discovered that he was suffering from a serious internal complication, involving a surgical operation, which was apparently successful. But evidently his strength, owing to his advanced years, was not sufficient to withstand the shock to the system, as he gradually became weaker from that time. His death occurred at the White House, Greenwich Park, on the evening of Saturday, January 2, 1892, in the ninety-first year of his age. Five days afterwards his remains were interred at Playford. EDWIN DUNKIN.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, January 8, 1892.

E. J. STONE, M.A., F.R.S., *Vice-President* (Past President), in the Chair.

The Chairman. I am sorry to say that the President is unable to be present this evening, and that both the Honorary Secretaries are unwell.

Mr. E. W. Maunder read the Minutes of the last Meeting, which were confirmed.

Mr. Maunder. 56 presents have been received since the date of the last meeting, but none of them call for particular notice.

A vote of thanks was accorded to the donors of the presents.

The Chairman. Everyone no doubt is aware of the lamented death of Sir George Airy, and I have been asked by the President to bring before the Fellows the resolution which has been passed by the Council, so that the Society may join in the expression of regret by the Council at the loss Astronomy has sustained. The resolution is as follows :—“That the Council [and Fellows *] of the Royal Astronomical Society desire to place on record their high appreciation of the eminent services rendered to Astronomy by the late Sir G. B. Airy throughout his long and active scientific career, and of the remarkable ability, zeal, and self-devotion with which he, as Astronomer Royal, directed the operations of the Royal Observatory during the long period of 46 years. That the President be requested to convey to the representatives of the late Sir G. B. Airy the expression of the sentiments of the Council [and Fellows *] and of their deep sympathy with his family in their bereavement.”

Mr. Ranyard. Perhaps, Mr. Chairman, it would be well that somebody should move that the Fellows should join in that expression of regret, and I shall consider it a privilege to do so.

Mr. Cottam. I will second that.

The resolution was agreed to unanimously.

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* The words in brackets were confirmed by the motion of Mr. Ranyard.

The Chairman. Then we will simply modify the address and include the Fellows in it.

Mr. H. H. Turner then read a paper by *Mr. E. E. Barnard* "On the phenomena of the transit of the first Satellite of Jupiter and observations on the planet."

"In a letter which I received from Mr. A. Stanley Williams he informs me that he has sent a communication to the Royal Astronomical Society in which he endeavours to explain the apparent duplicity of the first Satellite of Jupiter at its transit, September 8th, 1890, by supposing the phenomenon to have been a close conjunction of the Satellite with a small spot which he had seen three days earlier on the planet. It is wholly impossible that two experienced observers, Mr. Burnham and myself, should be so mistaken. The duplicity was watched from sidereal $18^h\ 30^m$ to $19^h\ 7^m$, and no relative motion detected, although a spot on Jupiter would have moved $0''\cdot15$ each minute of time—a displacement which would have been very apparent in a few minutes with the high magnifying-power employed on the 12-inch. Mr. Williams's explanation can have no bearing on the apparent duplicity observed. It should be accepted as unquestionable that the phenomenon of 1890, September 8th, was wholly connected with the Satellite, and one of the two explanations given in the 'Monthly Notices,' No. 9, vol. li., will doubtless be found in the end the true explanation. As soon as the transits of this object occur on a bright region we may expect to know something more definite.

"The new red spot which was the most striking feature in the southern hemisphere of Jupiter during the past opposition has disappeared. It was the most striking feature on the planet in October, was well-defined and clear red, but during November it faded rapidly, and on November 20th, when it was in transit at $8^h\ 2^m\cdot0$ Mount Hamilton mean time, and its longitude was $110^\circ\cdot9$, it was scarcely discernible. On December 14 no trace of it could be made out.

"The great red spot seems again slackening its rate of rotation. During the past opposition its longitude remained quite constant at about 3°, but its longitude is now increasing. On December 14th it was in transit at $5^h\ 5^m\cdot3$, and its longitude was $6^\circ\cdot4$. It is now very much more conspicuous than at opposition, and a stronger red in colour."

A vote of thanks was passed to Mr. Barnard for his paper.

Mr. Maunder read a paper on the "Reappearance of Saturn's Ring and position-angle before the disappearance, observed at the Observatory, Utrecht," by *Prof. J. A. C. Oudemans.*

The telescope of the Utrecht Observatory has an object-glass of Jacob Merz, aperture 0·260 metre, focal distance 3·200 metre. The eyepiece employed was a positive one of Steinheil with a magnifying-power of 162.

In 1891 October 28 and 29 Saturn was decidedly without ring, on October 30th the sky was overcast. At $17^h\ 30^m$ Utrecht mean time, October 31st, Saturn was visible for a moment through clouds; at both sides the ring was visible as a thin bright line. 1891, November 2nd, ring clearly visible with feeblest magnifying power 114. Dark line across planet visible on October 29th still visible. It seems to me this dark line must be the dark ring.

Twenty-eight observations were made with the wire micrometer in June and July 1891; the observed position-angles of the ring and the means are given for each day of observation with the 'Nautical Almanac' position-angles and corrections of the N.A. These corrections vary from $+26'{\cdot}95$ to $-16'{\cdot}6$.

After a careful discussion I adopted for the fourth edition of Kaiser's 'Sterrenhemel' the following values of the dimensions of Saturn and his rings at the mean distance 9·53885 (see vol. ii. p. 701 of that work):—

	Diameter.	Semidiameter.
	″	″
Outer Ring exterior	39·5	19·75
Inner Ring interior	29·5	13·75
Dusky Ring interior	21·8	10·9
Equatorial	17·3	8·65
Polar	15·4	7·7

If the mass of the bright rings is supposed to be $\mu \times$ mass of the planet, we deduce from these numbers that the thickness of the rings is $3''{\cdot}82\,\mu$. With $\mu=\frac{1}{118}$ (Bessel) this becomes $0''{\cdot}0324$. With $\mu=\frac{1}{314}$ (H. Struve) this becomes $0''{\cdot}0121$. The Sun seen from Saturn presents a disk$=3'{\cdot}355$; and the distance of the interior diameter of the bright ring being $5''{\cdot}1$, so long as both sides of the ring have sunshine the remaining shadow of the rings on the planet is $0''{\cdot}0274$ (Bessel's value of μ) or $0''{\cdot}0071$ (H. Struve's value of μ), whereas, according to Bessel's elements adopted in the 'Nautical Almanac,' the breadth of the dusky ring near the centre of the planet is $0''{\cdot}102$, a value much larger than either the $0''{\cdot}0274$ or $0''{\cdot}0071$, the breadth of the ring's shadow on the planet, adopting either Bessel's or H. Struve's mass of the ring.

A vote of thanks was passed to Prof. Oudemans for his paper.

Mr. W. H. M. Christie. "On the relation between diameter of image, duration of exposure, and brightness of object in Photographs of Stars taken at the Royal Observatory, Greenwich."

This paper contains a discussion of the measures of the diameters of disks of stars on certain experimental photographs which have been made at Greenwich with the Grubb photographic telescope, and deals with the time of exposure and magnitude of the star. It is, I am afraid, necessarily rather full of figures; but I will endeavour to give a general account of it. In the first place, before explaining the results arrived at in the paper, it will be well to exhibit on the screen some of the photographs which have been taken in order to deduce these results. In this investigation the point has been to take on the same plate a series of images with different exposures, in geometrical progression, and test the variations of the diameters with exposure and with the magnitude of the star. [The Astronomer Royal then explained photographs by means of the screen.] The result of the discussion of about 2000 measures of the diameters of 830 images is that the square root of the diameter varies as the logarithm of the exposure, or $\sqrt{diam.} = log.\ expos. + const.$, an empirical law which represents the observations through a range corresponding to 8 magnitudes with a mean error equivalent to only ±·05 magnitude. This formula is intermediate between the two which have previously been proposed, viz. (1) The diameter varies as a power of the exposure, and (2) The diameter varies as the logarithm of the exposure, neither of which satisfies these observations, as the curves show conclusively. That disposes of the first question, and shows how the diameter varies with the exposure. The next question is, how should the exposure vary with the brightness of the object photographed} to give equal photographic effects? The law generally assumed is that for equal photographic effects the ratio of brightness × exposure should be constant, and the discussion which I have made here leads to the conclusion that that is substantially true.

In this discussion I have made use of the valuable photometric determinations of 9th and 11th mag. stars recently made under Prof. Pritchard's direction at the Oxford University Observatory, which have proved of the greatest service, and for which I wish to express my obligation. I have also compared photographic images of 7th and 9th mag. stars with Argelander's magnitudes, the object in both cases being to find the factor by which the exposure should be multiplied to give similar images for stars fainter by 2 magnitudes, whether on the Oxford photometric scale from 9 to 11 mag. or on Argelander's scale from 7 to 9 mag. The results in the two cases are in remarkable agreement. There are, it is true, certain variations between the results obtained from the individual photographs, and, as is not surprising, considerable discordances between photographic determinations and visual determinations made with the photometer. If we have a star which is visually of the 9th mag., it may be photographically something like an 8th mag. on the one hand or a 10th mag. on the other. When we have such a large range as that, taking a comparatively small number of stars we cannot expect that we shall completely eliminate these disturbances. There are also accidental variations in the state of the sky, in atmospheric or instrumental tremors, in incipient dew on the object-glass, and in sensibility or development of different parts of the film, which may have their effect; so that it is not surprising that there are discrepancies. I will put down the results obtained from the different photographs, discussed by comparison with the Oxford 9th and 11th mag. stars and also with Argelander's 7th and 9th mag. stars.

Diff. of Mag. for Factor 2·512.

Photo.	Oxford mag.	Argelander mag.
128 ……	0·84	….
178 ……	0·90	….
180 ……	0·87	….
181_1……	1·27	….
181_2……	0·87	….
185_1……	1·04	0·82
185_2……		1·12
Means..	0·964	0·969

Now if the law were absolutely true, each of these results should be 1·00 mag., and I think everyone will agree that the means, whether we take the Oxford scale or Argelander's, are in as close accord with the law as could be expected. The comparison with Argelander may be considered as a rough check, which is very satisfactory as far as it goes. The results for Photo 181 were divided into two groups of diameters under and over $2''\cdot1$ respectively, and it is curious that the smaller diameters give a markedly larger factor, indicating that the first faint images of 11 mag. stars were more readily obtained relatively to those of 9 mag. than the denser images. Three of these photographs (Nos. 171, 180, and 181), before being measured for diameter, were carefully examined by me independently to see which images were shown with the several exposures. I find this result is corroborated; that is to say, that with the shorter exposures we get relatively better results with the 11th-mag. stars than we do with the 9th-mag. stars. On Photo 181 it is remarkable that faint images of several 11th-mag. stars were obtained in 40 secs., and on Photos 180 and 181 distinct measurable images were obtained in 100 secs. of all stars down to 11·1 and 11·2 mag. One point which has come out in these discussions is that a very slight deposit of dew on the object-glass may cause a great diminution in photographic intensity, rendering a much longer exposure necessary. Special care has therefore to be taken that the object-glass is quite clean and free from dew. As a check on this I have proposed that a supplementary exposure of 20^s should be given, which should show readily Argelander's 9th-mag. stars. A simple comparison with Argelander's map would then show if anything was wrong, and by this check we should have security, in the case of each photograph, that the 11th-mag. stars had been photographed.

Dr. Common. Are these compared with Argelander's maps or with the Oxford photometric measures?

The Astronomer Royal. With both. The first column is with

Oxford and the second with Argelander. I should not, however, lay so much stress on these latter. I think it is admitted that Argelander's magnitudes for stars fainter than the 9th mag. are not altogether trustworthy, and it is not easy to get a sufficient number of the brighter stars to give an accurate measure of the interval from 7th to 9th mag.; but so far as the comparison goes it shows as to these plates that this law substantially holds good.

The Chairman. There is another paper on a similar subject.

Mr. H. H. Turner then read a paper by *Mr. W. E. Wilson* on the subject of "A new Photographic Photometer for determining Star Magnitudes." Mr. Turner said that the paper was a description of an automatic arrangement which might be attached to a telescope for giving a series of exposures of 100^s, 63^s, $39^s.8$, and continuing in this ratio down to 1^s to any star the magnitude of which it was required to determine. The exposures are given automatically, and the plate is moved automatically $\frac{1}{10}$ of an inch between each exposure. A second series of exposures is then made on a standard star, say Polaris, a vertical movement of $\frac{1}{10}$ of an inch having been previously given to the plate. The plate is then developed, and the relative number of images of the two stars will give their magnitudes to 0·5. The times of exposure vary as the number whose log is 0·2, but there is no reason why they should not be made to give 0·1 magnitudes. The instrument will also be of use for determining the actinic value of the sky, for by taking a series of Polaris and finding at what exposure it fails to record itself, the exposure necessary to record a star of another magnitude will be known. The values of wire screens in front of the object-glass can also be determined. Mr. Wilson hopes shortly to exhibit before the Society some negatives taken with the instrument.

Mr. Ranyard. I do not know whether the Meeting or whether the Astronomer Royal has noticed it, but he did not mention that there is a very great difference in the sharpness of the edges of the star-disks corresponding to short and long exposures. It was very evident in the photographs thrown upon the screen that the longer the exposure the softer and more nebulous is the edge of the star-disk. The short exposures give very much sharper-edged disks, and that no doubt accounts for what the Astronomer Royal said, that the estimates of magnitude were better when derived from short than from the long exposures. I noticed this a long time ago, and recently on examining Dr. Max Wolf's photographs of the Milky Way, and comparing the stars with Prof. Pickering's spectroscopic catalogue, I found that there was a marked difference in the sharpness of the edges of the disks of stars having different types of spectrum, the first type of spectrum being naturally somewhat bluer or richer in rays at the ultra-violet end of the spectrum than stars having the solar type of spectrum. This is no doubt due to the fact that Dr. Wolf's photographic camera is not perfectly corrected for achromatism, especially for the ultra-violet region, but it enables one very readily to pick out these stars of the first or Sirius type of spectrum. He happened to be using a very much shorter focus lens, in which the chromatic dispersion is not as well corrected as in the lens which the Astronomer Royal has been using, and of course these effects were exaggerated. I have made some experiments, and long taken an interest in the question the Astronomer Royal has been discussing, and it seems to me that this law as to diameter of disks must be looked upon as an empirical one, it must change from instrument to instrument. When a bright star is outside the field of a telescope you see a bright region of the heavens coming into the field before the star itself appears, due to dispersion of light in the Earth's atmosphere, and possibly to want of polish on the object-glass. It is evident that a star-disk which is brightest at the centre degrades rapidly into an illuminated field. At first the decrease of brightness from the central image is very rapid, and afterwards it is more gradual. The size and brightness of the nebulous field about a star must depend upon the polish of the object-glass and upon the dispersion of light within the lenses as well as upon the good figuring of the lenses and upon even the clearness of the atmosphere.

With regard to the figures written down upon the blackboard by the Astronomer Royal, I was pleased to see that they bore out the law with regard to the intensity of the photographic image which I communicated to the Society in 1885. The figures really do not differ from unity by as large a percentage as Prof. Pritchard's star magnitudes differ from those of Prof. Pickering. I have closely compared the two catalogues, and there are frequent differences of $\frac{2}{10}$ of a mag., which corresponds to a difference of 50 per cent. in the amount of the light. Consequently I look upon the Astronomer Royal's results as confirming the law I stated as to the density of the photographic image and the time of exposure. If the Astronomer Royal wants to find some good determinations of the magnitudes of small stars, he will find several measures by Prof. Pickering of the stars near the Pole, in the Pleiades, and in other clusters.

Mr. Plummer. I think another reason can be assigned for the closer agreement of the measures of the smaller diameters with the theory proposed by the Astronomer Royal than that given by Mr. Ranyard, whom I have not been able entirely to follow. The larger diameters will increase much more slowly than the smaller, and errors of measurement will have a proportionately greater effect on the time of exposure required for their production. The Astronomer Royal will perhaps explain his method of measurement and say in what light he regards a diameter as the correct measure of photographic action. In whatever way the photographic image is propagated through the film, whether it be regarded as a species of fluorescence or scattered reflexion, there is, I imagine, a greater photographic action at the centre than at the periphery, and some error may arise in giving to the entire disk equality of actinic action. I am glad to find that there is a reasonable proposal of carrying Argelander's scale down to the 11th magnitude, at least in the work of the International Chart, and I am much obliged to the Astronomer Royal for the compliment he has paid the Oxford Observatory, and his acknowledgments of the efforts there made to secure this desirable end.

Mr. Turner. I may remind the Fellows of the importance of this question of the equivalence of exposure and intensity at the present time. In the early days of photography the connection between these two was assumed; I have not sufficient information as to when it was first suggested. Mr. Ranyard claims to be the author of the suggestion in 1884. I cannot now express an opinion on this point*. But we do remember that after this law had been adopted in such places as in the account of photography by Admiral Mouchez in the 'Annuaire du Bureau des Longitudes' for 1887, it was seriously questioned by Dr. Scheiner of Potsdam, when the work of the chart was about to be commenced. He said that from experiments of his own, an exposure of 2½ times only increased the stellar magnitude just visible by ½ instead of 1, and that we should have to expose for eight hours instead of half an hour if we wanted to get the 14th magnitude on the photographic chart. That was a very startling statement, and I was sceptical. However insufficient might be the evidence for the law, I felt that observers could not be so far wrong as that, and it is gratifying to learn that the laborious observations communicated by the Astronomer Royal have confirmed this law and that we must relegate Scheiner's experiments to the category of those affected with some hitherto unexplained source of error. The Fellows will be glad to learn, on seeing the paper of the Astronomer Royal in print, that the ladies who have so enthusiastically pressed themselves into the service of Astronomy at Greenwich have been responsible for much of the labour represented by this investigation.

Mr. Ranyard. Scheiner makes a most extraordinary assumption. He counts the number of the stars on his plate, and assumes that the number of the stars of successive magnitudes increases according to the space ratio. He founds the whole of his theory upon the assumption that the stars ought to increase in number according to the space ratio.

The Astronomer Royal. As regards the estimation of faint images and of denser ones, that is a point of very great importance which has to be carefully considered. What it practically comes to is this. We have a sort of central nucleus of dense deposit, and we have a penumbra or fringe more or less faint. Observers have been careful to measure the diameter not of the extreme penumbra, but about halfway between the penumbra and nucleus, so that the diameter should be that of the circle which, if of the same density as the nucleus, would represent the total photographic action. That estimate can be made very fairly with tolerable accuracy considering the circumstances, and we find observers agree pretty well in their estimates, so that there is not so much difference as might be supposed.

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* [*Vide* Correspondence on p. 123.—H. H. T.]

The Chairman. By measuring the diameter of the image you do not measure enough. You ought also to measure the density of the deposit which forms the star image.

Dr. Common. That is a very important point that Mr. Stone makes—that if you get the length and breadth you disregard the depth. You may have a thin deposit over the whole area of the image of a low mag. star and a much less density of deposit of the silver than you would have for a higher mag. star of practically the same diameter of image. In all these investigations it has only been the length and breadth that have been taken, and not the thickness.

The Astronomer Royal. Oh, yes; we have measured the thickness.

Dr. Common then gave a blackboard illustration.

The Astronomer Royal. I think you are taking quite different conditions from those with which we have to deal. From my point of view it is essential that the exposure should be fairly suited to the mag. of the star. If we use such exposures as to give very large images, I do not think we can expect very accurate measures. I think these *minutiæ* are very interesting, and deserve very careful consideration; but we may in practical work disregard them, I think, if, by measures of diameters, we get any scale that will be sufficient to solve the problem which we are concerned with. That is a sufficiently difficult problem I must say.

Mr. Isaac Roberts said that he finds the character of the photo-image of the stars on his negatives taken at Crowborough Hill differ considerably from the images taken with the same instrument at Maghull. He illustrated on the blackboard the difference referred to by showing the true star-image as a black spot surrounded by nebulous diffraction effects at Crowborough, whereas at Maghull stars of similar magnitude showed a stain of uniform density on the film without the black nucleus. These effects refer only to faint stars, for bright stars imprint an image which is not a small black spot, but a large black spot with a margin that is not clearly defined. He attributed the difference in the star images to greater clearness of the atmosphere at Crowborough than prevailed at Maghull, and also to the greater concentration of light by the reflector than by the photo-refractor. He contended that before the International Astronomers definitely committed themselves to an absolute formula for determining stellar magnitude from photographs they should carefully consider and formulate the instrumental, the atmospheric, and the chemical conditions. These will involve much laborious investigation.

A vote of thanks was passed to the Astronomer Royal for his paper.

Mr. Turner read a paper on the Effect of cutting off portions of Object-glasses. In 1886 (vol. xlvi. p. 329) he had described some simple experiments showing that the line of collimation of a telescope was altered by cutting off portions of the object-glass, his attention having been drawn to the matter by the discordance between readings for coincidence of collimators when the view was unobstructed or partially obstructed. This note had apparently induced Dr. Wislicenus, of Strasburg, to make a very complete series of experiments on the effect of using various screens, the result being to confirm Mr. Turner's conclusion that the phenomenon was purely optical. Dr. Wislicenus further suggested that it was due to a virtual change of focus when the screen was interposed, and a consequent parallactic effect. As the subject had attracted attention, Mr. Turner wished to lay before the Society some notes he had made a few years ago, though no very definite conclusions had been reached. The chief new fact was that the discordance found in regular collimation observations at Greenwich had, after remaining constant for ten years, changed either gradually or in two sudden steps. Figures were given showing the annual mean discordance from 1871 to 1891. No satisfactory explanation had been found of this change. The possible influences of change of observer, of the introduction of the reversion prism eyepiece, of gradual changes of azimuth of the collimators, &c. &c. had all been considered, with a negative result. It seemed most likely that the general figure of the lenses had changed. It must be remembered that the quantities under consideration were small from the optician's point of view, though quite sensible from the astronomer's.

A vote of thanks was passed to Mr. Turner for his paper.

The following papers were announced:—

Prof. J. A. C. Oudemans. "Reappearance of Saturn's Ring and Position-angle before Disappearance, observed at Utrecht."

Royal Observatory, Cape of Good Hope. "Observations of Occultations of faint Stars during the Lunar Eclipse on 1891, Nov. 15."

H. H. Turner. "On the Observations for coincidence of the Collimators through the Cube of the Greenwich Transit-circle."

J. Gledhill. Phenomena of Jupiter's Satellites &c., observed at Mr. Crossley's Observatory."

E. E. Barnard. "On the Phenomena of the Transit of the First Satellite of Jupiter, and observations of the Red Spots on the Planet."

Prof. S. Newcomb. "On the Dynamics of the Earth's Rotation, with respect to the Periodic Variations of Latitude."

Dr. R. Copeland. "Observations of the Total Eclipse of the Moon, 1891, Nov. 15, at Edinburgh."

W. E. Wilson. "A new Photographic Photometer for determining Star-magnitudes."

W. H. M. Christie. "On the Relation between diameter of Image, duration of Exposure, and brightness of Object in Photographs of Stars taken at the Royal Observatory, Greenwich."

J. Gledhill. "Observations of Double Stars made at Mr. E. Crossley's Observatory, Halifax."

Rev. T. E. Espin. "The Red Stars in the great Perseus Clusters."

Dr. D. Gill. "On the Investigation of the Division Errors of the Scales of the Repsold Measuring-apparatus at the Cape Observatory, and the Determination of the Errors of the Oxford Réseau."

A. Marth. "Ephemerides of the Satellites of Saturn, 1891–92." (Continued.)

Royal Observatory, Greenwich. "Observations of Occultations of Stars by the Moon and of phenomena of Jupiter's Satellites made in 1891."

E. J. Stone. "Note on some Values of the Sun's mean horizontal Parallax which have been deduced from the Transit of Venus Observations made in 1882."

Nautical Almanac Office. Ephemeris of Juno near the time of Opposition, 1891, from the corrected Elements published in Monthly Notices, vol. l. p. 495."

Edward Herbert Lees, Orbost, Victoria, Australia; *J. de Mendizábal Tamborrel*, 13 Calle de Jesús, Mexico; *Prof. John Samuel Slater*, Civil Engineering College, Seebpore, Calcutta; *Arthur Thornton, M.A.*, Giggleswick School, Yorks.; *Wm. Livingstone Watson*, Ayton, Abernethy, Perthshire, were duly elected Fellows of the Society.

The following Candidate was proposed for election as a Fellow of the Society:—

Arthur Gibbons, Science, Art, and Technical School, Brierly Hill, Dudley (proposed by J. C. Roger).

Notes on New and Old Nebulæ.

On November 7, 1890, I picked up a pretty bright, pretty large nebula, 2° S.E. of γ Camelopardi, which I supposed to be one of Swift's faint objects, No. 1469 of the New General Catalogue. But on again sweeping up the former nebula on Dec. 4, 1891, I was induced to ascertain its position more accurately, and found that it differed from the place of any known object. Nos. 1469 and 1485, discovered by Swift on Feb. 24, 1886, are near, but the new object lies between them and is a far more conspicuous object. It is curiously situated relatively to telescopic stars, lying between two pairs, N. and S.; and one of the components of the N. pair is the 8½ mag. star, D.M. +69°235. A line of about half a dozen very faint stars on the W. side of the nebula points to it. So far as the experience gained in comet-seeking enables me to judge, I should say that there is not another nebula so plain as this remaining to be discovered north of the equator. Its place in 1890 is:—

$$\alpha\ 3^h\ 56^m\ 20^s;\ \delta\ +69°\ 30'.$$

When observed with a power of 145 on my 10-inch reflector, the nebula appears mottled, and I suspect there are some very minute stars involved with it, or that the nebulosity is irregularly broken up into knots. The proximity of the bright telescopic star (only 4′ distant on its N. side) may possibly have been the cause of the nebula having previously escaped discovery.

On August 9, 1891, I swept up a faint, small nebula in Cepheus. It precedes the star D.M. +79°750 (mag. 7) $1^m\ 32^s$ of R.A. (=4′ of arc) in the same declination, so that its place for 1890 is:—

$$\alpha\ 22^h\ 41^m\ 33^s;\ \delta\ +79°\ 51'.$$

I find this rather a difficult object, except on a good night, though I picked it up with a power of only 40. It is noteworthy as being situated in the midst of a region containing very few nebulæ. There is no similar object in the N. G. C. within 10° of it, but Prof. Swift writes me that he has recently discovered a very faint, small nebula in $\alpha\ 23^h\ 41^m\ 45^s$, $\delta\ +75°\ 2'\ 45''$, which is about 6° distant from the one observed here. The latter has a very faint double star on its N. border; the components are just visible with a power of 252.

On Sept. 30, 1891, I met with a tolerably bright, pretty small nebula in the position—

$$\alpha\ 3^h\ 31^m\ 13^s;\ \delta\ +76°\ 18'\ (1890).$$

It precedes the star D.M. +76°137 (mag. 8·2) $7^m\ 3^s$, and is in same declination. There is an extremely faint star involved on the S. side of the nebula. The latter is fairly conspicuous with a power of 60, and much brighter in the middle. This object is also new, for I find nothing corresponding to it either in the N. G. C. or in Swift's recent catalogues published in 'Ast. Nach.'

N. G. C. 896 (H. III. 695) is described as "e F, p L, i F," and as "pretty large," implies a diameter of 50″ to 60″; the description does not agree with several observations I have obtained of this object with my 10-inch reflector. I not only see Herschel's faint nebula, but another feeble mass of nebulosity involving small stars closely east and possibly connected with N. G. C. 896.

In Sept. 1802, Sir W. Herschel discovered "three nebulæ in a line and 1′ distant from one another" (N. G. C. 3210, 3212, 3215). The position of the central one is $\alpha\ 10^h\ 16^m\ 35^s$, $\delta\ +80°\ 23'{\cdot}3$ (1890). He described two of them as very faint and small, and the one preceding as stellar. D'Arrest re-observed the two former, and I also picked them up on Sept. 11, 1890; but with regard to 3210 there are two or three faint stars in the place assigned for it, but, like D'Arrest, I see no nebulosity.

N. G. C. 6951 and 6952 really refer to the same object, as there is only one pretty bright nebula near the positions given for them. It has a faint star closely following it, and on July 27, 1891, I ascertained the place of the nebula by differential observations with 4 Cephei. Comparing this with the positions given by Swift and Coggia, and bringing them up to 1890, we have:—

No.	Observer.	α			δ		
		h	m	s	°	′	
6951………	Swift (II. 85)…	20	35	39	+65	43·3	p B, p L, l E.
— ………	Denning ……	20	35	40	+65	40·3	p B, p L, E; *13 mag. f.
6952………	Coggia ………	20	36	15	+66	3·2	p B, oval, diffused; *15 mag. close f.

Swift apparently includes the same object in his Catalogue IV., where it figures as No. 47, and his position, reduced to 1890, and description are:—

	α	δ	
Swift (IV. 47) ……	$20^h\ 35^m\ 57^s$	$+65^\circ\ 41'.2$	p B, p S. R, m b M; p B * near.

Swift's places and descriptions are not quite accordant, and they disagree with Coggia's results, which are probably erroneous. My determination of July 27, 1891, corroborates Swift's II. 85 (=N. G. C. 6951), and I believe this is accurate, and that all four of the above observations relate to one and the same object. It would be well to re-examine this spot. N. G. C. 6953 lies near; but this is exceedingly faint and difficult, and not likely to cause any confusion as to the identity of the object or objects described above.

W. F. Denning.

Bristol, 1892, Jan. 15.

On the Intensity of the Images of Stars as affected by Wire-gauze Screens placed before Object-Glasses.

During the last few months my attention has been directed, in connection with the International Star Chart, to the effect of wire-gauze screens on the visual and photographic images of stars. The details of these researches seem hardly fit for communication to the 'Observatory,' but a rapid account of the results may be interesting to all whose attention is directed to telescopes.

The screens in question consisted mostly of close rectangular meshes, and the diffraction images were such as are well known in physical optics.

I believe that this is the first time since the discovery by Fresnel of these curious effects, that the actual intensity of the light of the central image has been measured, or, in fact, could have been measured by existing instruments; but the law of this intensity, in relation to the nature of the screen, had been long ago *computed.* This law I have now by actual observation verified, and certain curious results have accompanied the verification.

The instruments used were the photographic telescope of 13 inches aperture, the large directing telescope attached to it, of nearly the same aperture, and the De la Rue reflector. The photometer was the wedge usually employed in this observatory.

On applying a series of gauze screens to these three telescopes, it is found:—

1. When applied to the visual telescope the intensity of the central star-image varies, very approximately, as the square of the area of light admitted by the screen. This is in strict conformity with the law derived from the usual Diffraction Theory.

2. When applied to the photographic Object-Glass, the relative proportion of light lost in the photographic image is very much greater than in the case of the same screen placed before the visual O.G.; but here, again, so long as the photographic O.G. is unchanged, the law of the squares of the effective light areas is still very approximately observed, when different screens are applied.

I attribute this considerable, but by me unexpected, loss of light to the fact that the O.G. employed was so constructed as to mainly collect the actinic G rays in the focal field, while in the visual telescope many rays of various characters are effective in the production of the star-images.

3. In order to test this supposition, the same screens were applied to the De la Rue reflecting telescope, in which the images in the focal field are necessarily the same, whether that field is used visually or photographically. The result was a continuity of the law of the squares of the light-areas, even in passing from the visual to the photographic intensities. In fact there was very approximately the same loss of light caused by the same screen, whether the mirror was used visually or with a photographic plate.

4. The object of the foregoing research was to find a screen which should intercept two magnitudes of light, and thus convert a ninth magnitude star apparently into an eleventh.

There was comparatively no great difficulty in finding such screens, but from the foregoing investigation it appears that the effects do not depend on the screen alone, but are connected more or less with the particular O.G. to which they are applied. Moreover, it was found that the necessarily very fine meshes of these screens are liable to be clogged with dust and injuriously affected by the moisture. In lieu of these screens, photographic type specimens and catalogues of stars of magnitude eleven have been furnished from this Observatory to the several astronomers concerned in the International Chart. The details of the observations, of which this communication is a brief abstract, will be found in the 'Comptes Rendus' of the French Academy of Sciences for 1891 December 28. C. PRITCHARD.

University Observatory, Oxford.

Double Stars for February.

THE following interesting double stars are well placed for observation in February. They may all be observed with a telescope of moderate aperture. The positions are for 1890.

Σ 1175. R.A. $7^h\ 57^m$, Dec. +4° 28′; Mag. 7·9, 9·1.

Slow direct motion.

1888·19	221°·6	1″·99	Leavenworth.
1888·23	221 ·40	1 ·86	Tarrant.

Σ 1187 = 85 Lyncis. R.A. $8^h\ 3^m$, Dec. +32° 34′; Mag. 7·1, 8.

Slow retrograde motion.

1829·50	71°·0	1″·61	Struve.
1878·11	53 ·1	(2 ·35)	Doberck.
1881·20	49 ·2	2 ·17	Bigourdan.
1888·28	225 ·11	2 ·02	Tarrant.

ζ Cancri = Σ 1196. R.A. $8^h\ 6^m$, Dec. +17° 59′.

A well-known triple star; mag. 5·3, 5·5, 5·8. Period of close pair 59 years (Seeliger).

AB	1889·19	40°·3	1″·05	Leavenworth.
AC	1889·19	119 ·3	5 ·65	"
BC	1889·19	128 ·5	5 ·43	"

Burnham 208. R.A. $8^h\ 34^m$, Dec. −22° 16′; Mag. 7, 8.

Probably a binary. Measures desirable.

1874·20	30°·4	1″·4	Burnham.
1889·15	47 ·5	1 ·06	"

ε Hydræ = Σ 1273. R.A. $8^h\ 41^m$, Dec. +6° 50′;
Mag. 3·8, 7·8.

Slow direct motion. The primary star is a very close pair.

1887·64	226°·83	3″·44	Tarrant.
1889·08	226 ·5	3 ·16	Burnham.
1889·13	228 ·9	3 ·21	Leavenworth.

Σ 1338. R.A. $9^h\ 14^m$, Dec. +38° 40′; Mag. 7, 7·2.

Slow direct motion. Apparently neglected by observers in recent years. Measures are desirable.

1829·53	121°·1	1″·76	Struve.
1876·21	149 ·56	2 ·16	Doberck.
1876·3	151 ·8	1 ·63	Dunér.

Σ 1348. R.A. $6^h\ 19^m$, Dec. $+6^\circ\ 47'$; Mag. 7·3, 7·4.
Retrograde motion, but very slow.

1831·02	334°·3	1″·09	Struve.
1888·26	325 ·59	1 ·77	Tarrant.
1888·62	145 ·0	1 ·73	Leavenworth.

γ Leonis. R.A. $10^h\ 14^m$, Dec. $+20^\circ\ 24'$; Mag. 2, 3½.
Binary. Period 407 years (Doberck).

1888·35	114°·0	3″·36	Schiaparelli.

σ^2 Ursæ Majoris = Σ 1306. R.A. $9^h\ 1^m$, Dec. $+67^\circ\ 35'$; Mag. 5, 8·2.
Slow retrograde motion. Probably binary.

1881·09	238°·3	2″·40	Bigourdan.
1884·28	235 ·14	..	„

J. E. Gore.

The recent Disturbance in the Northern Hemisphere of Jupiter.

Some account of the recent remarkable eruption of dark spots on the north temperate belt of Jupiter has already been given by Mr. Denning in the 'Observatory,' No. 180, p. 330, and by the writer in No. 181, p. 361. As this disturbance appears to have aroused considerable and widespread interest, it is proposed to give here some additional particulars, bringing the history of the subject up to date.

At the present time the disturbance has in great measure subsided, as although there are still a number of spots visible, they are mostly inconspicuous and difficult to observe. But a great change has come over the appearance of the north temperate belt itself. In the early part of the apparition extremely faint and inconspicuous, this belt has become broader and more prominent than the north equatorial belt, and is only inferior to the great south equatorial belt. In September and October there were a great many small blackish spots visible on the southern edge of the belt, most of which, however, have now disappeared. But three of the largest and most conspicuous spots were made the object of special study, and the knowledge now accumulated concerning them renders them objects of peculiar interest. Lists of the times when these spots might be expected in mid-transit were prepared and circulated from time to time amongst several observers, and by this means they have been kept continuously under observation up to the present time. The writer is greatly indebted to Mr. Denning of Bristol, and the Rev. A. Freeman of Sittingbourne, Kent, for a number of valuable observations of the times of transit of these spots.

The first of the three spots above mentioned is that to which Mr. Denning called attention in the 'Observatory,' No. 180, p. 330. It is proposed here to call it *a*. Its early history has already been sufficiently described. The second spot, *b*, resembled *a* in appearance, but was smaller and less conspicuous. It was first observed on August 19, and from that time up to September 15 its rotation was perfectly uniform, and performed at the rate of $9^h\ 49^m\ 1^s\!\cdot\!6$. But on the latter date its motion received a sudden check, similar to that which occurred in the case of the spot *a* at the same time, as already described (see 'Observatory,' No. 181, p. 361), but even greater in amount. From the 15th September up to the 3rd November the period of rotation was $9^h\ 49^m\ 33^s$ (119 rotations), an increase of no less than $31^s\!\cdot\!4$ over the old period *.

The third spot, *c*, was first observed on the 14th May, 1891, and is particularly interesting on account of its large and conspicuous character, its great degree of permanency, and from the rapidity of its motion. Between May 14 and Sept. 15 the rotation was performed in a period of $9^h\ 49^m\ 5^s\!\cdot\!5$. On the latter date, however, like the other two spots, its motion received a sudden check, by which the period of rotation was increased by $13^s\!\cdot\!3$ to $9^h\ 49^m\ 18^s\!\cdot\!8$. Later on a slow and gradual increase occurred in the velocity of the motion of this spot, by which the duration of a rotation was again reduced, so that we have the following different results:—

May 14–Sept. 15 rot. per = $9^h\ 49^m\ \ 5^s\!\cdot\!5$ (303 rotations).
Sept. 15–Oct. 20 „ = $9^h\ 49^m\ 18^s\!\cdot\!8$ (85 rotations).
Oct. 20–Dec. 27 „ = $9^h\ 49^m\ \ 6^s\!\cdot\!2$ (166 rotations).

The remarkable change which occurred on Sept. 15 is particularly worthy of attention on account of its peculiar character. The three spots were rotating at very different rates, and between *a* and *c* there was an interval of 93° of zenographical longitude. Yet the motion of all three spots was suddenly and simultaneously affected.

Quite recently a further change has occurred in the rotational rates of spots *a* and *b*, which appears to be of quite a different nature to that which took place on Sept. 15. Owing to the different relative motions of the three spots they gradually approached each other. On the 15th Sept., as already stated, there was an interval of 93° of longitude between *a* and *c*. By December 11 these two spots had drawn so near to each other, that this interval was reduced to only 28°, and if the relative motions had remained unaltered, the former must have been overtaken shortly by the latter. But spot *a* now began to move more swiftly, the rotation period between December 9 and January 21 being $9^h\ 49^m\ 14^s.3$. Between October 3 and December 9 the duration of a rotation had been $9^h\ 49^m\ 39^s.4$, so that the decrease in the length of the rotation amounts to $25^s.1$. Accordingly we have the following different results :—

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* The effect of these changes will perhaps be more clearly comprehended by the following figures. An alteration of 30^s in the period of rotation represents a change in velocity of over 500 miles a day, or 22 miles an hour.

Aug. 21–Sept. 15 rot. per = $9^h\ 49^m\ 27^s.2$ (Denning, 61 rotations).
Sept. 15–Oct. 15 ,, = $9^h\ 49^m\ 44^s.2$ (73 rotations).
Oct. 3–Dec. 9 ,, = $9^h\ 49^m\ 39^s.4$ (163 rotations).
Dec. 9–Jan. 21 ,, = $9^h\ 49^m\ 14^s.3$ (105 rotations).

It would seem that in this case spot *c* overtaking the more slowly moving mass of material represented in part by spot *a*, and pushing behind, has forced the latter to move on more swiftly, a change which appears to be very different in its nature from that which occurred on September 15. Spot *b*, lying between *a* and *c*, it may be mentioned was affected in similar manner to *a*. It would thus seem that the mass of material constituting *c* had greater influence than that forming the two spots *a* and *b* together, since whilst causing such a considerable change in these two spots, the motion of *c* itself remained to all appearance unaffected.

The following observed times of transit of spot *a* will complete the lists of observations already given in the 'Observatory.' Observers:—B. = Mr. E. E. Barnard; D. = Mr. Denning; F. = Mr. Freeman; W. = Williams. The times attributed to Mr. Barnard have been derived from his valuable paper in the November No. of the 'Monthly Notices.'

Date.	G.M.T. of Transit. h	m	Obs.	Date.	G.M.T. of Transit. h	m	Obs.
1891, Sept. 18..	18	26·1	B.	1891, Nov. 27..	9	17·9	W.
Oct. 4 ..	17	45·4	,,	Dec. 9..	6	20·2	F.
,, 7 ..	14	36·8	,,	,, 11..	7	32·8	W.
,, 29 ..	7	33	D.	,, 20..	7	32·4	,,
Nov. 3 ..	5	25	,,	,, 27..	6	31·2 ±	,,
,, 7 ..	7	40·9	F.	,, ,, ..	6	35·8	F.
,, ,, ..	7	46·5	W.	1892, Jan. 21..	5	49·3	,,
,, 27 ..	9	17·3	F.				

The following are a few selected observations of Spot *c* :—

1891, May 14..	15	41·5	W.	1891, Nov. 5..	8	12·3	W.
Aug. 15..	12	21	,,	,, 30..	7	6·5	F.
Sept. 2..	12	16	D.	Dec. 11..	8	18·7	W
,, 15..	14	22·8	W.	,, 27..	7	18·6	,,
Oct. 5..	15	30·9	B.	1892, Jan. 21..	6	23·6	F.

The spots hitherto described have all been situated on the southern edge of the dark north temperate belt. This belt is actually double, being composed of two parallel bands lying so close together, that in a small telescope they would be seen as a single belt. Some spots which have been visible on the northernmost band of this double belt have behaved very differently to those situated on the southernmost band. Instead of rotating in a period of little more than $9^h\ 49^m$, their rate of rotation has approximated to that of the red spot ($9^h\ 55^m\ 40^s$). Consequently we have here the very remarkable instance of two atmospherical currents running side by side, and differing in velocity by about 280 miles per hour. And yet these currents lie so close together, that in a small telescope the one would appear confounded with the other. In the early observations some rather puzzling discordances arose from observing the spots independently of their position on the north or south edges of the belt. It is possible that the difficulty experienced by Mr. E. J. Sells in identifying the spots observed by him ('Observatory,' No. 184, p. 59) may be due partly to a similar cause. In very poor seeing it was difficult sometimes to exactly fix the situation of a spot.

Although the spots on the north side of the north temperate belt rotated in general at approximately the same rate as the red spot, in one instance there was a remarkable temporary divergence from this rule. A large protuberant mass lying on the north side of the belt was observed to move rapidly westward with respect to the red spot for a period of 40 days, after which it appears to have become stationary with regard to it. From September 9 to October 20 the period of rotation of this protuberant mass was as short as $9^h\ 54^m\ 31^s{\cdot}3$ (99 rotations). Whilst during the 9 days between the latter date and the 29th October the velocity of its motion decreased by 50 miles an hour. The following observations, with the corresponding longitudes according to Mr. Marth's Ephemeris (System II.), will show clearly the rapid temporary motion westward of this marking;—

Date.	G.M.T. of Transit.		Longitude.
	h	m	°
1891, Sept. 9 ……	12	16·1	311·3
„ 23 ……	13	2·7	284·7
„ 28 ……	12	0·2	278·7
Oct. 3 ……	10	50	267·9
„ 15 ……	10	14·1	249·7
„ 17 ……	11	47·6	246·8
„ 20 ……	9	10	242·2
„ 29 ……	11	36·5	242·8
Nov. 3 ……	10	49	245·1
„ 13 ……	8	58·3	240·0

All these interesting changes show that this region of the planet, usually rather a quiescent one, has been lately in a state of intense and abnormal disturbance.

A. STANLEY WILLIAMS.

1892, Jan. 23.

Selenographical Notes.

Unrecorded Craters west of Billy and Hansteen.—One of the many indentations in the bright lunar surface east of Gassendi, due to the irregular outline of the southern side of the Mare Procellarum, forms a partially enclosed area of inconsiderable size, bounded on the west by the tongue of hilly country east of Letronne, and, towards the limb, by the two associated circular ring-plains, Billy and Hansteen, each about 30 miles in diameter; the former remarkable for the abnormally dark tone of its level interior under all conditions of illumination (in which respect it is hardly surpassed by the duskiest parts of Grimaldi or Riccioli), and the latter for the many features on its floor, and for the remarkable clefts on its eastern *glacis*.

Dr. L. Weinek, the Director of the Royal Observatory at Prague, having recently detected two craters on this area which do not appear in any published map or drawing of the locality, the attention of observers is especially directed to it. They will be found a little north of an imaginary line ranging from the north-west wall of Billy to the small object, *b* (east of Letronne), in Neison's Map XII., the more westerly standing at about one-third of the distance between *b* and the wall, and the other a trifle more than midway between its companion and the same point. In Schmidt's Map, Section XX., the position of the first is approximately 49°·2 E. long. and 12°·6 South lat. Through Dr. Weinek's courtesy, I received in October last a reproduction of his very truthful and beautiful drawing of the region, dated 1890, April 1, $8\frac{1}{2}^h$ to 11^h, Prague M. T., showing the two craters in question under a low morning Sun, the east wall of Hansteen being on the terminator. In this they clearly appear as crater-cones, with black centres, casting definite shadows on the grey surface of the Mare, on which, with the exception of a small ill-defined spot on the north-east of the more westerly of the pair, they are the only visible objects for many miles round.

Unfavourable weather prevented me from examining the neighbourhood till January 11, this year, 7^h to 9^h, when the definition, after being only moderately good at first, ultimately became excellent, so that the two ring-plains and the surrounding details were admirably displayed, notwithstanding the somewhat advanced stage of illumination which prevailed on the site. At 7^h four small, faint, light, circular spots with ill-defined borders were easily seen, two of them occupying the positions shown in his drawing and situated as just described, while the other two were placed crosswise,—one on either side of a line joining the first pair, and consequently ranging nearly north and south. On examining them again at $8^h\ 25^m$ with a power of 284 on my $8\frac{1}{2}$-in. Calver reflector, a glance was sufficient to show the first-mentioned objects as tolerably distinct craters with dusky (not black) interiors and very white but indefinite borders, while with 350 these characteristics were so apparent that it would be difficult for any observer examining the region with ordinary attention to overlook them. I may add, that, later on, my 4-in. Cooke-achromatic, which is mounted on the same declinition-axis as the reflector, was brought to bear on these objects with a power of 200, when they were just recognizable as craters. No indications of dark centres were visible in the two companion spots, which have a general resemblance to the row of three faint little white dots on the plain farther north, west of Hansteen. Schmidt shows a group of eight or nine more or less elongated hills near the place occupied by the two craters and the neighbouring spots,—all drawn as prominent objects, three of them especially so. Mädler draws two *hills* which very nearly agree as regards position with the two craters in question, the more westerly being represented as an oval-shaped elevation standing on the west side of a light area on the Mare,—other hills figuring some distance to the north of them. Neison closely follows Mädler. If the region is observed at full, or examined in a good photograph, representing the Moon at this phase, it will be found that the site of Dr. Weinek's craters is occupied by two whitish patches,—that towards the west being much the larger and exhibiting an extension towards the north in the form of a long irregular white marking without any definite outline. This extension is prominently displayed in the Rutherfurd and Lick photograms and indicates rather a difference in the reflective power of the surface than the existence of hills or other similar physical features. The curious fan-shaped mountain *a*, west of a line joining the centres of Billy and Hansteen, is preeminently the most brilliant feature in the photographs of the neighbourhood.

Additional observations of this region, made under as many different conditious of illumination as possible, are very desirable. Observers will also be doing good service by searching for any notes or drawings they may possess relating to these ring-plains and the vicinity which will tend to throw light on the discrepancies between Schmidt's and other charts and the observations just recorded. Schmidt shows a crater some little distance beyond the north-west wall of Billy, which may possibly, though very improbably, represent the more easterly of the craters, greatly misplaced; but neither his map nor any others I have seen, exhibit any feature which can be taken to represent the companion on the west which was observed soon after sunrise as a crater-cone by Dr. Weinek, and subsequently by myself, under a much higher angle of illumination, as a crater with a dusky centre. T. Gwyn Elger.

Beaumont House, Shakespeare Road,
Bedford, 1892, Jan. 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Bright Spots on Saturn's Equatorial Belt.

Gentlemen,—

In No. 181 of the 'Observatory' (Nov. 1891) Mr. W. F. Denning has directed attention to the bright spots on Saturn's equatorial belt seen in 1891 by Mr. A. S. Williams, and has deduced from that observer's published observations a mean period of rotation for these spots, which is in fact $10^h\ 14^m\ 26^s{\cdot}6$. I have not specially looked for these spots, but have seen similar ones on four occasions.

1891 Oct. $30^d\ 18^h\ 32^m$ (G.M.T.).—A bright round spot was seen entering on the following limb of Saturn, within the bright equatorial belt *north* of his equator. It was kept in view till daylight approached. (1)

Nov. $1^d\ 17^h\ 58^m$.—An elliptic segment, concave to N., was as it were cut out of the *north* or crape-ring-edge of the dark rings crossing Saturn, as if by a bright elliptic spot at the central meridian upon the *north* equatorial bright belt. (2)

Dec. $10^d\ 18^h\ 32^m\ 15^s$—A bright spot was just wholly within the following limb of Saturn upon the bright equatorial belt *south* of the dark rings crossing the planet. The spot was well on the planet 44 minutes afterwards. (3)

1892 Jan. $3^d\ 18^h\ 45^m$.—A bright spot was seen on the *north* bright equatorial belt. It slightly preceded the centre. (4)

The first two observations with 6-inch, the other two with 6½-inch refractor. I cannot identify these spots with those indicated by the *published* observations of Mr. A. S. Williams. But the positions of the four spots observed by me, relatively to Mr. Marth's zero meridian ('Monthly Notices R. A. S.' vol. li. no. 9, Supplement, 1891), would be about as follows:—(1) in longitude 222°·5, (2) in long. 19°·6, (3) in long. 270°·9, (4) in long. 282°·3. So that the spots appear to be all different ones. Mr. Marth's adopted period of rotation is $10^h\ 14^m\ 21^s{\cdot}84$, which he says represents the motion of the spots observed by Mr. A. S. Williams.

During the first half of the present year it is probable that many observations of similar spots may be noted. It may then be possible to determine whether they preserve fixed positions relative to each other; and whether the rotation-period, derived from spots on the *south* part of the equatorial bright belt, agrees with that derived from spots on the *north* part of that bright belt.

Yours faithfully,

A. Freeman.

Murston Rectory, Sittingbourne,
1892, Jan. 20.

Periodical Comets due in 1892.

Messieurs,—

La lettre que l'hon. M. W. T. Lynn vous adressait en date du 7 décembre 1891 relativement aux "Periodical Comets due in 1892" (*The Observatory*, January 1892, p. 59) omet, au nombre des comètes dont le retour doit être attendu cette année, la 1^re^ comète périodique de Tempel, 1867 II. Dans le No. de janvier 1891 de *The Observatory*, p. 62, M. Lynn signalait en revanche cette comète comme devant revenir déjà dans le courant de l'année dernière. Ceci demande un mot de rectification que je prends la liberté de vous envoyer.

La 1^re^ comète périodique de Tempel a été observée à ses apparitions successives de 1867, 1873 et 1879. Elle avait alors une durée de révolution de 6 années. De 1879 à 1885 elle a, comme du reste durant la période de 1867 à 1873, subi de très fortes perturbations de la part de Jupiter. Ces perturbations ont profondément modifié son orbite et prolongé la durée de sa révolution de plusieurs mois. D'après les calculs que j'avais faits et que j'ai publiés dans le No. 2656 des *Astr. Nachrichten* (vol. cxi. p. 241) elle a dû passer à son périhélie le 25 septembre 1885. Malheureusement on ne l'a pas retrouvée cette année là, par suite d'un concours de circonstances défavorables. Elle reviendra dans le courant de 1892 et comme elle a cheminé, depuis 1885, à grande distance de Jupiter, on peut admettre, sans grande erreur, qu'elle passera à son périhélie dans les premiers jours d'avril de cette année.

Dans une note que je viens d'adresser aux *Astr. Nachrichten*, je donne une éphéméride approchée pour le retour de la comète. D'après mes calculs elle se trouvera à sa plus faible distance de la terre au commencement du mois de juin, mais cette distance minimum sera encore supérieure à la distance moyenne de la terre au soleil.

Il faut cependant espérer qu'il sera possible d'observer la comète de Tempel cette année, ce qui serait fort important pour la connaissance de sa théorie.

Veuillez agréer, Messieurs, l'expression de ma considération très distinguée. R. Gautier.

Observatoire de Genève,
le 12 janvier, 1892.

The 'Nautical Almanac.'

Gentlemen,—

The amount of information given in the 'Nautical Almanac' has of course been greatly increased since it was started by Maskelyne in 1766. But its early years contained more than some foreign astronomers seem to have been aware, if we may judge by a remark of Schröter in the 'Philosophical Transactions' for 1792. In a paper "On the Atmospheres of Venus and the Moon," dated Lilienthal, April 10, 1792, and read (translated) on the 24th of

May, he says: "The following observation of an occultation of Jupiter by the Moon, when near its full, occurred to me by mere accident on the 7th of April, 1792; and I was the more gratified by it, as I had long wished for an opportunity to observe such a phenomenon, and had no expectation of seeing it at this time, no mention being made of it in the Ephemerides."

The occultation in question was, however, duly predicted in the 'Nautical Almanac,' $10^h\ 5\frac{1}{2}^m$ and $11^h\ 9\frac{1}{2}^m$ being assigned as the times of immersion and emersion respectively. The 'Berliner Jahrbuch' for the year does not mention the occultation; but it is given in the 'Connaissance des Temps.' Yours faithfully,

Blackheath, 1892, Jan. 18. W. T. Lynn.

Bright Meteors.

Gentlemen,—

The following bright meteors have recently been observed here:—

No.	Date. 1891.	h	m	Mag.	Path. From α	δ	To α	δ	Probable Radiant.
1	Dec. 5	9	45	♃	251°	+66°	264°	+46½°	Geminii.
2	Dec. 20	8	38	♀	124	+64	159	+49	14° +7°
3	Dec. 31	6	57	1	17	+70	330	+61½	97 +29
	1892.								
4	Jan. 3	11	57	♀	144	− 5½	147½	−10	Auriga or Perseus.
5	Jan. 11	8	5	♀	21	+60	318	+34	97°+29°

Notes.

1. Motion very slow. Radiant-point doubtful; path not well observed.

2. Very slow. Meteor descended almost vertically, and was apparently brightest in the early part of its path. Radiant probably near ε Piscium, and the same as that of the fine meteor of Sept. 30 last ('Observatory,' Nov. 1891, p. 379).

3. Very slow. Meteor pear-shaped, though not very brilliant. The nucleus threw off sparks in its flight. Radiant at about 97° +29°, and possibly same as that of No. 5.

4. Seen imperfectly. The meteor was observed just as it passed behind the corner of a building; the length and direction of path are therefore inaccurate. A fine object, giving a bright flash at disappearance.

5. A large meteor seen in moonlight. It moved slowly, duration 7 seconds. Path nearly vertical—nucleus star-like at first; at the end it burst into a stream of luminous fragments.

These meteors were observed casually, and their recorded paths are therefore not so exact as they might otherwise have been. If any of them were noticed at other stations I should be glad to receive particulars, with a view of computing the real paths.

Bishopston, Bristol, Yours faithfully,
1892, Jan. 14. W. F. Denning.

PUBLICATIONS.

The Solar Parallax and its Related Constants.—This is the title of a work forming Appendix III. to the Washington Observations for 1885, in which Prof. W. Harkness collects all the observations hitherto made by anybody anywhere of anything connected with the Solar Parallax, and mixes them for the extraction of a refined essence—a homogeneous system of new values. It is a gigantic piece of work, and does not so readily lend itself to criticism, [which to be adequate would involve a labour of comparable dimensions] as to admiration. The computations were all made by the author himself with the help of a *Thomas* arithmometer only; but so far from finding so much arithmetic irksome, Prof. Harkness seems to have revelled in it; indeed he occasionally could not resist the temptation of using as many figures as the arithmometer would hold, though he prints them half-apologetically with the remark that "the accompanying probable errors will suffice to prevent these decimals from creating any false impression respecting the degree of accuracy attained."

The process of adjustment by least squares discovers the lacunæ in the material available. A compendious table given by Prof. Harkness, in which he clearly shows that he understands the vanity of using many figures when one will do, shows at a glance the relations between the twelve variables which he selects as fundamental. These are:—

p, the constant of solar parallax.
P, the constant of lunar parallax.
$\mathfrak{P}$, the constant of luni-solar precession.
$\mathfrak{N}$, the constant of nutation.
Q, the parallactic inequality of the Moon.
L, the constant of the Earth's lunar inequality.
a, equatorial semi-axis of the globe of the Earth, if that body is regarded as a spheroid, or major equatorial semi-axis if it is regarded as an ellipsoid.
θ, the time taken by light to traverse the mean radius of the Earth's orbit.
V, the velocity of light per second of mean time.
E, the combined mass of the Earth and Moon.
M, the mass of the Moon.
$\epsilon = (a-b) \,/\, a =$ the quantity variously designated as the ellipticity, compression or flattening of the Earth.

The following table shows as clearly as possible the relative importance of each quantity in determining the probable error of every one of the twelve quantities included in the general adjustment.

"The several quantities are entered symbolically in the first column of the table, and on the same line with each of them the principal sources of its probable error are indicated in the order of their importance by the numerals 1, 2, 3, etc. For example, in determining the probable error of Q, the parallactic inequality of the Moon's motion, the quantities whose observed values have the most influence are, 1, the constant of aberration; 2, the solar parallax; 3, the velocity of light; 4, the mass of the Earth; and, 5, the light equation. A glance at the table shows that the numeral 1 occurs once in each of the columns P, 𝔓, V, and *ε*, three times in the column L, and five times in the column *a*. It is therefore evident that in order to improve the system of constants

TABLE XX.—Origin of the Probable Errors of the Adjusted Quantities.

Quantities.	Principal sources of the Probable Errors.											
	p	P	𝔓	𝔑	Q	L	*a*	*θ*	V	E	M	*ε*
p	2	...	...	...	...	...	1	...	3	4	...	...
P	...	1	...	...	...	3	...	...	...	...	...	2
𝔓	...	...	1	...	...	...	...	...	...	...	...	...
𝔑	...	...	...	2	...	1	...	...	...	...	...	...
Q	2	...	...	...	...	...	1	5	3	4	...	...
L	...	3	...	2	...	1	...	...	...	...	4	...
a	2	...	...	...	...	...	1	4	...	3	...	...
θ	2	...	...	...	...	...	1	4	...	3	..	...
V	...	...	...	...	...	...	...	...	1	...	...	...
E	2	...	...	...	...	...	1	...	3	4	...	...
M	...	3	...	2	...	1	...	...	...	...	...	...
ε	...	2	...	4	...	3	...	...	...	...	...	1

determined in the present paper the quantities whose observed values we should endeavour to improve are, 1, the constant of aberration; 2, the lunar inequality of the Earth's motion; 3, the solar parallax; and, 4, the constant of nutation. The constant of precession and the velocity of light are so well determined as to be virtually independent of all other quantities, while the parallactic inequality of the Moon and the mass of the Moon from the tides are so uncertain that they have little influence upon anything.

"On account of the great importance of the constant of aberration in our adjustment, it seems desirable to recall the fact that at present we have no satisfactory theory of aberration. The constant in question has long been regarded as the ratio of the Earth's mean orbital velocity to the velocity of light, but notwithstanding the plausibility of that assumption it has never been rigorously deduced from the undulatory theory of light.

"The desiderata for the improvement of the system of constants discussed in the present paper may now be recapitulated as follows:

"1. The parallax of the Moon should be determined by the diurnal method, at one or more stations as near as possible to the equator.

"2. The observatories in the northern and southern hemispheres should co-operate with each other for two or three years in systematically making meridian observations of the Moon to improve our knowledge of its parallax.

"3. Pendulum experiments should be made at a large number of stations, located partly in the neighbourhood of the equator and partly as near as possible to the poles. Experiments in middle latitudes are also desirable, but somewhat less necessary.

"4. New determinations of the constants of aberration and nutation should be made by as many different methods as possible.

"5. The meridian observations of the Sun, accumulated at the Greenwich and Washington Observatories during the last fifty years, should be discussed in such a way as to deduce from them the most probable coefficient of the lunar inequality of the Earth's motion.

"6. New determinations of the solar parallax should be made by observing Mars during its opposition in 1892, and also by observing such asteroids as may come into favourable positions for that purpose.

"7. The measurement of some of the great arcs included in the scheme of the U. S. Coast and Geodetic Survey should be completed as soon as possible.

"There can be no doubt that the observations specified in paragraphs 1, 2, and 3 would materially improve our knowledge, both of the lunar parallax and of the Earth's flattening; but the probable errors of the constant of aberration, the constant of nutation, the coefficient of the lunar inequality of the Earth's motion, and the solar parallax are already so small that it will be exceedingly difficult to reduce them any further. Nevertheless, the attempt should be made."

BULLETIN OF THE ASTROPHOTOGRAPHIC CHART. — Admiral Mouchez has issued in advance an extract from the next volume containing a summary of the experiments made with screens before the object-glass. It was resolved at the last meeting of the Permanent Committee that, "in order to enable observers to pass in a uniform and sure manner from the 9th magnitude of Argelander to the magnitude 11, which is desired for the plates of the Catalogue, a Sub-Committee shall distribute to the participating Observatories screens of metallic gauze, absolutely identical for all Observatories. These screens placed before the objective of the photographic telescope, shall diminish the magnitude of a star by two units, and in the determination of the diminution of magnitude, the members of the Sub-Committee shall adopt the coefficient 2·512 for the ratio between the brightness of two consecutive magnitudes."

This resolution was due mainly to a statement by Dr. Scheiner that an increase of exposure in the ratio 2·512 could by no means be assumed equivalent to passing from one magnitude to the next. Experiments which he described seemed to show that the corresponding gain of magnitude was little more than 0·5 instead of 1·0. The members of the Committee had no counter evidence of the truth of the principle to offer, and adopted Dr. Scheiner's view that some other method of passing from magnitude 9·0 to magnitude 11·0 should be used. Dr. Vogel, whom we may assume Dr. Scheiner to have represented as well as himself, promptly supplied Admiral Mouchez with 18 gauze screens, which he said would reduce the light of a star by two magnitudes, and these were distributed among the different Observatories. But on trial neither these nor other screens gave satisfactory results. Light which traverses such screens "seems to suffer, besides ordinary diffraction, certain modifications which the Committee did not foresee."...."The effect produced is very variable, according to the particular circumstances, the state of the sky, the quality of the object-glass, and even the colour of the stars, and as yet it has not been found possible to determine the cause of such differences."

But this unsatisfactory result is fortunately not all. The attention of observers has been drawn to the verification of the law that duration of exposure is equivalent to intensity of light, and sufficient evidence has now been accumulated to allow of its adoption in place of the discarded screens; and such is Admiral Mouchez's concluding recommendation.

The evidence given in the report is that of some experiments by the Brothers Henry on artificial stars photographed by a portrait-lens. More valuable than this, is the evidence accumulated by the Astronomer Royal on real stars, and using the 13-in. photographic objective at Greenwich (see p. 96).

We can only hope that Admiral Mouchez's suggestion will be adopted. And indeed we would go further. We should like to see the time of exposure fixed at a definite number of minutes, whatever the weather. Experience has shown that it is very difficult to judge of the photographic quality of a night; and there are sure to be great errors of judgment in aiming at uniformity in the limiting magnitude obtained. To fix the exposure absolutely in time would save much trouble and anxiety; render the work much simpler and more fitted for the more or less unskilled photographers to whom it may have sometimes to be entrusted; and the gain or loss in limiting magnitude would only be a few tenths. Instead of trial plates to estimate the quality of the night, a permanent automatic record of the quality could be kept by exposing a plate in a fixed small camera pointing to the pole, and letting Polaris trail. This plate would be developed with the Chart plates next morning, but would require little or no attention from the observer. Intermittent clouds would show by breaks in the trail, and the width of the trail would be the required index to quality.

We should have said that Dr. Vogel does not agree with his colleagues. He considers the 18 screens he sent out satisfactory, and equivalent to two magnitudes, and the fact that they represent an increase of exposure in the ratio 1 to 10 or 12 for most other object-glasses would probably not affect this opinion, for he has not disavowed Dr. Scheiner's conclusion that exposure must be increased in a much greater ratio than 1 to 6¼ to obtain two magnitudes.

We may perhaps quote the remark of Mr. Ellery which led Admiral Mouchez to refer to the effect of the colour of a star:—

"We have already exposed several plates to try the screen, and in one of them we found that two of Argelander's stars, magnitudes 8·7 and 9·2, are always on the plate exposed without a screen, but with the screen the star 8·7 is no longer there, while the other is strongly shown. Several plates have been tried to verify this fact, and all have given the same result. This star is very faintly orange-coloured."

H. H. T.

NOTES.

Comet Notes.—The following ephemeris of Barnard's Comet (Oct. 2, 1891) is by Frœbe, and differs slightly from that previously given.

Berlin Midnight.

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
Feb. 2....	16	19	6	32	21	Feb. 8....	16	22	4	30	19
4....		20	14	31	41	10....		22	50	29	37
6....		21	13	31	0	12....		23	39	28	55

Brightness 0·13 on Feb. 12.

Ephemeris of Wolf's Comet. Berlin Midnight.

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
Feb. 1....	4	29	29	9	44	Feb. 17....	4	44	58	7	1
5....		32	57	9	3	21....		49	27	6	21
9....		36	42	8	22	25....		54	7	5	42
13....		40	43	7	41	29....		58	59	5	4

Brightness 1·65 on Feb. 3; 0·97 on Feb. 27.

Ast. Nach. No. 3072 contains an interesting account of observations of the transit of Wolf's Comet over 21 Asterope in the Pleiades made at the Lick Observatory on Sept. 3, 1891. The difference of declination of this star and 22 Asterope was taken at short intervals before, during, and after the transit, to determine if possible the refraction produced by the comet. The result, however, is so small that all we can say is that no displacement exceeding 0″·2 took place. The star approached within 10″ of the nucleus, and the diameter of the head was estimated at ¾′ to 1′.

Brook's Comet, 1890 *a*, was seen by Javelle at Nice, on Jan. 6, nearly two years after its discovery. The place is stated to agree closely with Bidschof's ephemeris.

As Winnecke's Comet may be picked up shortly, we give the following ephemeris by Dr. Haerdtl. It is for Berlin Midnight:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
Feb. 1....	12	44	24	16	4	Feb. 21....	12	52	44	20	48
5....		46	50	16	48	25....		53	2	22	4
9....		48	55	17	39	29....		52	46	23	26
13....		50	37	18	36	Mar. 4....		51	53	24	54
17....		51	55	19	39	8....		50	21	26	28

At Mr. Ranyard's request we print the following correspondence:—

11 Stone Buildings, Lincoln's Inn, W.C.,
Jan. 9, 1892.

Dear Sir,—

Your remark last night* left me with the impression that you thought I was claiming a little more than I was entitled to. I therefore enclose a copy of my paper in the 'Monthly Notices' read at the December Meeting, 1885; and I should like to draw your attention to the report of the Meeting and to my letter in the 'Observatory' of April 1889, p. 184. It is unpleasant to me to make reclamation, but when my facts are denied and four years after claimed as something new from the same quarter, I think that I am entitled to have the law spoken of as mine rather than as a discovery of the physicist who had forgotten that he denied it.

Yours truly,

H. H. Turner, Esq. A. Cowper Ranyard.

Royal Observatory, Greenwich,
1892, Jan. 11.

Dear Mr. Ranyard,—

Thank you for your letter and enclosure.

I hope you will find that I shall be prompt to acknowledge my errors, if you will kindly give me a day or two to look up the matter thoroughly as you suggest. It was a new claim to me, though it should probably not have been so; and I found it difficult to instantly fall in with the new view. Please do not misconceive my attitude.

Yours very truly,

H. H. Turner.

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* *Vide* p. 101.

1892, Jan. 16.

Dear Mr. Ranyard,—

A few moments of unexpected leisure this morning enabled me to look up your references.

I think it would tend to clear up our points of difference if I state briefly my notions of what would constitute the 'discovery' of a law. There are two stages—

(1) The formulation of the idea.

(2) The experimental proof.

Now the formulation of the idea in the present instance can probably not be assigned to any one. The equivalence of brightness and exposure probably occurred to many people very early in the history of photography. It had certainly occurred to Capt. Abney before your paper was read in 1885, for he made a statement obviously summarizing his experience. I do not think you would wish to claim the law as your own from this point of view.

(2) As regards the experimental evidence, experiment is always a question of approximation, and I must say I am very much inclined to share Capt. Abney's doubts as to the conclusive character of your experiments. He describes them as "rude," and obviously meant to imply that his own experiments were of a more refined character; and therefore his qualified statement "I think I may say that double brightness &c." involves really two—(A) that he knew well enough that brightness was *roughly* equivalent to exposure; (B) but that on carrying the investigation to a higher degree of accuracy results seemed to be against the *absolute* equivalence.

I cannot see that his finally being led to alter statement (B) gives you any claim to priority, because you halted (in *his* opinion; I fear I am not able to examine the evidence as an expert) at the rough equivalence which he admitted.

Would you care for me to state anything of your letter or of my position in print? Yours truly,

H. H. Turner.

Capt. Abney's remarks are reported in the 'Observatory' for 1886 January, p. 52, as follows:—

"I consider that the experiments made by Mr. Ranyard are rather a rude way of getting the relative values of exposures. It by no means follows that more exposure and increase of brightness are synonymous. This is a very important point and but little attended to; but I think I may say that a double exposure is not equal to the double brightness of an object."

Another Cold Year.—The year just closed adds another to the series of years of each of which, on the whole, the mean temperature has been below the general average. Previous references to this question will be found in the 'Observatory' for April and July

1889, pages 196 and 301. It may be interesting to give here the mean yearly temperature of the air at the Royal Observatory, Greenwich, for each of the forty years 1849 to 1888, as deduced from the photographic records, and included by Mr. Ellis in his recent paper "On the Mean Temperature of the Air at the Royal Observatory," adding thereto the values for 1889, 1890, and 1891, and also the departure of the temperature of each year from the general average of the forty years 1849–1888.

Year.	Mean Temp.	Depart. from Average.	Year.	Mean Temp.	Depart. from Average.	Year.	Mean Temp.	Depart. from Average.
	°	°		°	°		°	°
1849...	50·3	+0·8	1864...	48·9	−0·6	1878...	49·7	+0·2
1850...	49·4	−0·1	1865...	50·8	+1·3	1879...	46·3	−3·2
1851...	49·4	−0·1	1866...	50·3	+0·8	1880...	49·5	0·0
1852...	50·7	+1·2	1867...	49·1	−0·4	1881...	48·8	−0·7
1853...	48·0	−1·5	1868...	52·0	+2·5	1882...	49·8	+0·3
1854...	49·2	−0·3	1869...	49·9	+0·4	1883...	49·4	−0·1
1855...	47·2	−2·3	1870...	49·1	−0·4	1884...	50·7	+1·2
1856...	49·3	−0·3	1871...	48·9	−0·6	1885...	48·7	−0·8
1857...	51·3	+1·8	1872...	51·0	+1·5	1886...	48·8	−0·7
1858...	49·6	+0·1	1873...	49·3	−0·2	1887...	47·9	−1·6
1859...	51·2	+1·7	1874...	49·7	+0·2	1888...	47·8	−1·7
1860...	47·5	−2·0	1875...	49·8	+0·3	1889...	48·0	−0·7
1861...	49·2	+0·3	1876...	50·6	+1·1	1890...	48·7	−0·8
1862...	49·9	+0·4	1877...	49·9	+0·4	1891...	48·4	−1·1
1863...	50·7	+1·2						

For the last seven years it will be seen that the values have been constantly below the average. There was a tendency to cold in the early part of the series, a tendency to warmer years at the middle of the series, and later again a tendency to cold. To show the distribution of cold through the year, during the last seven years, the following table is given, the first quarter including January, February, and March, and so on :—

	Departure from Average Temperature.				
Year.	1st Quarter.	2nd Quarter.	3rd Quarter.	4th Quarter.	The Year.
	°	°	°	°	°
1885	+0·2	−0·8	−1·4	−1·3	−0·8
1886	−3·4	−0·6	+0·9	+0·5	−0·7
1887	−2·5	−1·3	+0·5	−2·9	−1·6
1888	−2·9	−1·5	−2·9	+0·4	−1·7
1889	−1·7	+1·3	−1·6	−0·7	−0·7
1890	+1·4	−0·2	−1·1	−3·2	−0·8
1891	−2·4	−1·5	−1·3	+0·8	−1·1
Mean Temp. 40 years...	40·0	53·0	60·6	44·3	49·5

Thus out of 28 quarters 20 were below the average and only 8 above. The last four months of 1891 were all above the average, the first instance since the year 1886 of as many as four months consecutively being above the average. The continued low temperature in late years has been found by Flammarion, in his 'Revue Mensuelle' for April and May, 1891, to have been general also over great part of Europe.

It had been supposed by some persons that the growth of London had influenced injuriously the Greenwich temperatures as regards their indication of secular change. But this appears not to be the case; for from a comparison which Mr. Ellis has made of the Greenwich temperatures with the mean of the temperatures of the country stations reporting to the Registrar-General, situated between latitudes 51° and 52°, it appears that the mean Greenwich temperature for the 20 years 1869–1888 is 0°·43 lower than the mean for the 20 years 1849–1868, which is precisely the amount by which the mean for the Registrar-General's stations 1869–1888 is also below the mean for 1849–1868, the difference being thus identical in each case.

ROYAL METEOROLOGICAL SOCIETY.—The Annual General Meeting of this Society was held on Wednesday evening, the 27th January, at the Institution of Civil Engineers, 25 Great George Street, S.W.; Dr. W. Marcet, F.R.S., Vice-President, in the Chair.

The Report of the Council for the past year showed the Society to be in a very satisfactory position. In May the Library and Offices were removed to more commodious premises at 22 Great George Street. After defraying the cost of fitting up the new offices, and the increased rental, there still remained a balance in hand of £224. Thirty-four new Fellows were elected during the year, the total number on the roll of the Society now being 552.

Owing to the absence of the President, Mr. Baldwin Latham, M.Inst.C.E., through an attack of influenza, his Address on "Evaporation and Condensation" was read by the Secretary.

The question of evaporation is as of great importance as the study of the precipitation of water on the face of the earth, as the available water supplies of the country entirely depend upon the differences between these two sets of observations. The Earth receives moisture by means of rain, dew, hoar-frost, and by direct condensation. It loses its moisture very rapidly by evaporation. Although evaporation mainly depends upon the difference between the tensional force of vapour due to the temperature of the evaporating surface, and the tensional force of the vapour already in the atmosphere, yet it is largely influenced by the movement of the air and by its dryness, or the difference between the dew-point and the actual air temperature. Evaporation goes on at night so long as the water surface is warmer than the dew-point. With sea-water the evaporation is about 4½ per cent. less than with rain-water, while with water saturated with common salt the evaporation is 15 per cent. less than with rain-water.

In his experiments, Mr. Latham used an evaporating-gauge made of copper, 1 foot in diameter, and containing one foot in depth of water, which was floated by means of a hollow copper ring placed 6 inches distant from the body of the evaporator, and attached to it by four radial arms. This form of evaporator was found extremely convenient in carrying on all evaporation experiments; it was floated in a tank 4 feet in diameter, containing 30 inches depth of water. During the period of 13 years, from January 1879 to December 1891, this evaporator has never once been out of order or been interfered with in the slightest degree by frost.

Experiments were made with some 5-inch evaporators as to the effect of colour on the amount of evaporation, one being painted white, another black, and the results given by these gauges were compared with a copper gauge exposed under similar conditions. This comparison was the means of showing that the greatest errors in evaporating-gauges arise from the capillarity of the water rising on the sides of the gauge and thus inordinately increasing the amount of evaporation. Consequently a small gauge, having a larger amount in proportion of side area than a larger gauge, gives a very much greater amount of evaporation.

The results from the floating evaporator, one foot in diameter, show that the average amount of water evaporated annually during 1879–91 was 19·948 inches. It was found, however, that, as a rule, during the period from October to March, there were certain occasions when condensation was measured. The amount of these condensations in 13 years averaged ·308 inch per annum. The 5-inch evaporating-gauge, freely exposed to atmospheric influences, gave during the same period (1879–91) an average annual depth of evaporation equal to 38·185 inches.

The average annual evaporation during the three years 1879–81 from the 5-inch copper gauge standing in water was 27·90 inches, from one painted black 22·97 inches, and from another painted white 21·74 inches, whilst a gauge of the same dimensions freely exposed in the atmosphere gave, during the same period, 36·96 inches, and the 1-foot floating evaporator 19·40 inches. The 5-inch copper gauge gave a larger amount of evaporation than the gauge painted black.

Mr. Latham next described some percolation experiments which were carried out by Mr. C. Greaves at Old Ford, by Messrs. Dickinson and Evans at Hemel Hempstead, and by Sir J. B. Lawes and Dr. Gilbert at Rothamsted. He then detailed the results of his own experiments, and also the gaugings of the underground waters in the drainage areas of the rivers Wandle and Graveney.

He further stated that in the course of his observations on the flow of underground water he had observed that at certain particular seasons of the year it was possible to indicate the direction and volume of the flow of underground streams, even when they were at considerable depth, owing to the formation of peculiar lines of fog.

Dr. C. Theodore Williams was elected President for the ensuing year.

Figure of Jupiter.—Prof. W. Schur has made an admirable series of measures of the diameter of Jupiter in different position-angles with the Göttingen heliometer (Ast. Nach. No. 3073). The effect of personality was eliminated by the use of a reversion-prism eyepiece, all measures being made in both the horizontal and vertical (apparent) directions. The disk is found to be a sensibly true ellipse, with diameters 37″·4 and 35″·0, a flattening of 1/15½. Vertical measures exceed horizontal systematically by 0″·3.

We are glad to note the enthusiasm with which South Australians have been celebrating the Jubilee of Mr. Charles Todd's public service. "It has been said that democracies are ungrateful," says a local paper; "but South Australians have at any rate no lack of appreciation of the eminent services rendered by Mr. Todd. . . . Full of years and honours, enjoying the confidence of the Government, the respect and goodwill of the public, and the esteem and even affection of his officers, he has reached his jubilee, and the end of his successful career is still in the future." It is pleasant to note that Mr. Todd's public life commenced at the Royal Observatory, Greenwich, in 1841.

On 1891 September 3, Wolf's periodic comet transitted the seventh-magnitude star 21 Asterope. Messrs. Burnham and Barnard took the opportunity of observing differences of declination between 21 and 22 Asterope, for detection of possible refraction by the cometary matter. Mr. Barnard's measures with a 12-inch refractor show no sensible change during the transit, but Mr. Burnham's with the 36-inch equatorial seem to show a slight change in the difference of declination, which might be lost in the smaller instrument in the accidental errors of observation.

No. 3073 of the 'Astronomische Nachrichten' contains Dr. Berberich's compilation of the occultations of stars by planets in the year 1892. For possessors of large telescopes this list must be very valuable, and even small instruments may be able to observe the occultation of Cord. Zone 2803 (mag. 7·5) by Mars on March 6, and the near approach of δ Capricorni (mag. 3) on October 24 and ι Aquarii (mag. 4·5) on November 3 to the same planet, and the occultation of 73 Piscium (mag. 6·0) by Jupiter on May 23.

Minor Planet No. 315 has been named Constantia. Dr. M. Wolf discovered No. 323 at Heidelberg by photography, on a plate taken 1891 December 22; and we hear by telegram that he discovered No. 324 on 1892 January 20, possibly also by photography.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY

No. 186. MARCH. 1892.

ANNIVERSARY MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, February 12, 1892.

Lieut.-General J. F. Tennant, C.I.E., R.E., F.R.S., *President*, in the Chair.

Secretary: A. M. W. Downing, M.A.

Mr. Downing read the Minutes of the last Anniversary Meeting, which were confirmed.

Mr. W. H. Maw, in the absence of the senior auditor Mr. Lecky owing to illness, read the Auditors' Report, which stated that they had examined the Treasurer's accounts for the year 1891, and found and certified the same to be correct. The cash in hand on Dec. 31, 1891, amounted to £452 4*s*. 7*d*.; the funded property of the Society had been increased by the purchase of £212 9*s*. 2*d*. New 2½ °/₀ Stock. The books and vouchers were examined and found to be satisfactory.

The President. Gentlemen, it is customary at these Meetings to proceed at once with reading the notices of the more conspicuous deceased Fellows; but this occasion is exceptional, and I propose to treat it as such. In the first three weeks of this year, as you all know, we have lost two of our most eminent members. Of one, Sir George Airy, I need say nothing, because it has been possible to write a notice of him which you will have in the usual way; but as regards Prof. Adams the case is different, and it has been impossible to follow the usual course. It has been thought that we should not omit at this Meeting, though we could not give formal notice of it, to commemorate in some way his name and the great service he has rendered to astronomy. Mr. Adams first became known in connection with his researches into the cause of the observed irregularities of the motion of Uranus. Having satisfied himself that these could only be explained by the effects of the attraction of an unknown outer planet, he devoted himself to determining its probable elements and furnishing astronomers with such indications as would lead to its detection. It has long been universally acknowledged that the information he gave would have sufficed for this purpose, though Leverrier's investigations, of which he was ignorant, having the same object, were published first, and led to the actual detection of the planet at Berlin in a place not far from that indicated by Prof. Adams. From that time he devoted himself to gravitational astronomy. Having determined the elements of the orbit of Neptune from observations, he took up the Moon's motion; and we owe to him many valuable papers on this subject, especially on the secular acceleration, where he first showed that a satisfactory agreement between the accepted theoretical value of acceleration and that determined from observations did not exist. This result was sharply contested, but it is now universally admitted to be correct. He demonstrated the identity of the orbit of the November meteors with Tempel's comet by showing that, of the possible periods which had been found for the time separating the passage of the most dense part of the meteor ring through its node, one only, that nearly identical with the period of the comet, was consistent with the observed motion of the node of the meteor ring, and that in fact the comet was a dense part of the ring. He gave the world a great number of other papers on various points of lunar theory from time to time, and it is believed he was lately engaged on a revision of the theory of Jupiter's satellites. We very much hope that this work will be completed. I am sure there could be no greater testimony of our gratitude to Prof. Adams than to finish his work, and in the best possible way to put it before the public.

Mr. Downing read extracts from the seventy-second Annual Report of the Council.

The Council regret that they have to record the loss by death of the following Fellows and Associates during the past year:—

Fellows: Sir G. B. Airy, Prof. J. C. Adams, Joseph Beck, H. L. Boulton, Dr. F. E. Brünnow, Captain W. Chimmo, Albert Escott, T. H. Hovenden, John Merrifield, J. S. Nimkey, N. R. Pogson, Henry Pratt, and E. W. Snell. *Associate*: Eduard Schönfeld.

Memoirs of Eduard Schönfeld, abridged from an article by Prof. Krueger in 'Vierteljahrsschrift,' Jahrgang xxvi. Heft 3; of Norman Robert Pogson, by J. L. E. Dreyer; and a full memoir of Sir George B. Airy by H. H. Turner were read.

Some extracts from Reports of Observatories and from Notes on some points connected with the progress of Astronomy during the past year were also read.

The President. Gentlemen, there is so heavy a ballot to-day by post that I think it will be desirable for the scrutineers to get to work early upon it. The probability is it will take them some time to clear off the list, and I will nominate Mr. Drower, Mr. Inwards, and Mr. Cushing as scrutineers. If it is your pleasure we will take the ballot at once so far as it goes, and then keep it open for a short time. I hear there are from 160 or 170 votes by post.

Mr. Ranyard proposed that the Ballot should close at half-past four.

Admiral Sir Erasmus Ommaney. I will second that.

The motion was carried unanimously.

Mr. Stone. Now that the statement cannot influence the result of the election, I wish to say that, although Mr. Maunder's name appears on the official voting list as one of the Secretaries, he is not in the slightest degree answerable for its appearance. Mr. Maunder took no action whatever to secure this nomination; and if it was wrong for the two names which appear on the list to have been nominated together, the fault is certainly not Mr. Maunder's, but the fault of those who brought him forward. Still Mr. Maunder says that if elected he will do his best for the Society. But neither by personal canvass nor by the action of his friends did he influence in the slightest degree his nomination. This much, I think, in justice to Mr. Maunder should be known.

The ballot was then taken.

The President. The Council have awarded the Society's Gold Medal to Prof. G. H. Darwin for his work on Tides and their influence on the figures and motions of the heavenly bodies.

The President then delivered his address, laying before the Society the grounds on which the award has been founded.

Sir Erasmus Ommaney. I beg to move the following Resolution: "That the Report now read be received and adopted, and that it be printed and circulated in the usual manner, together with the President's address and the Report of the Auditors." The applause with which the Report has been received shows that it has met with general satisfaction. We have also to thank the President for his very learned exposition of the subject to which his address referred. With these observations I beg leave to propose the motion.

Mr. J. P. G. Smith. I beg leave to second that.

The Resolution was agreed to.

The President. Gentlemen, at the last meeting of the Council a vote of condolence with Prof. Adams's family was passed, and I believe it was held over for the concurrence of the Fellows, as in the case of Sir George Airy's vote. This is the vote of the Council: "The Council of the Royal Astronomical Society desire to place upon record their high appreciation of the brilliant services which were rendered to Astronomy by the late Prof. John Couch Adams and to offer their sincere condolence to his family in their bereavement." If Fellows would like to join in this vote, which I think they would, I will ask that one of the Fellows will move that they be included in it.

Prof. Darwin. May I ask, as Prof. Adams's colleague at Cambridge, to be allowed to propose that?

Mr. Freeman. And may I be permitted to second Prof. Darwin's motion?

The Resolution was unanimously adopted.

The following was the result of the ballot:—

President: Mr. E. B. Knobel. *Vice-Presidents*: Mr. W. H. M. Christie (Astronomer Royal), Dr. J. W. L. Glaisher, Mr. E. J. Stone, Lieut.-Gen. J. F. Tennant. *Treasurer*: Dr. A. A. Common. *Secretaries*: Mr. E. W. Maunder, Mr. H. H. Turner. *Foreign Secretary*: Dr. William Huggins. *Council*: Capt. W. de W. Abney, Prof. Arthur Cayley, Hon. Sir James Cockle, Mr. A. M. W. Downing, Mr. George Knott, Mr. Frank McClean, Mr. W. H. Maw, Mr. W. E. Plummer, Mr. A. Cowper Ranyard, Mr. Isaac Roberts, Rev. Walter Sidgreaves, Dr. E. J. Spitta.

Mr. Maw. I beg to move that the thanks of the Meeting be given to the President, Vice-Presidents, Secretaries, and Members of the Council, who now retire, for their services in promoting the objects of the Society.

Dr. J. G. Petrie. I have very much pleasure in seconding that.

The Resolution was adopted.

Mr. Bryant. I propose a vote of thanks to the scrutineers for the trouble they have taken in conducting the ballot.

Mr. T. Buckney. I beg to second that.

The motion was agreed to.

Bertram Bennett, B.A., Paignton, South Devon; *Charles Bright, F.R.G.S., Assoc.M.Inst.C.E., M.E.E.*, Telegraph Works, Silvertown, Essex; *Charles Burckhalter*, Chabot Observatory, Oakland, California, and 962 Chester Street, Oakland; *Arthur Hill Molesworth, B.A.*, 15 Park Lane, W.; *R. A. Sampson, B.A.*, St. John's College, Cambridge; *Charles Daulman Webb, B.A., B.Sc.*, King's College School, and 112 Adelaide Road, South Hampstead, N.W., were duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

Louis Joyner, Bermuda (proposed by J. McCarthy); *Belgrave Ninnis, M.D., F.R.G.S.*, Deputy Inspector-General, Royal Navy, 46 Kensington Park Road, W. (proposed by Capt. W. J. L. Wharton); *John Krom Rees, A.M., E.M.*, Director of the Observatory and Professor of Practical Astronomy and Geodesy, Columbia College, New York City (proposed by Prof. A. W. Wright); *William James Watson*, Gentleman, Morley House, South Stockton-on-Tees (proposed by Rev. W. Birks); *Elizabeth Brown*, Further Barton, Cirencester (proposed by Capt. W. Noble); *Alice Everett, M.A.*, 8 Gloucester Place, Greenwich, S.E., and Royal Observatory, Greenwich (proposed by A. M. W. Downing); *Annie Scott Dill Russell*, 16 The Circus, Greenwich, S.E., and Royal Observatory, Greenwich (proposed by Edward W. Maunder).

BRITISH ASTRONOMICAL ASSOCIATION.

The third ordinary Meeting of the second session was held in Barnard's Inn Hall on Wednesday, January 27th, Capt. Noble, the President, in the chair. Seventeen new members were elected, and a list of five candidates for membership was read.

Among the presents received during the month were a number of comparison-spectra of the Sun and various metals photographed by Mr. McClean, and the first number of the new American publication entitled 'Astronomy and Astrophysics.'

The President stated that the latter was the old 'Sidereal Messenger' revived under a new name.

Mr. Maunder remarked that Mr. McClean's photographs were not only beautiful but of great scientific value. He also said that the new publication would be a record of the spectroscopic work of the world.

Sir Howard Grubb showed a small equatorial fitted with an arrangement by means of which the declination and right ascension were read off on circles around the eyepiece of the telescope. He had also fitted to the same instrument a rotating carrier to hold several eyepieces.

Mr. Evershed then read a paper upon "The Distribution of the Solar Prominences in Latitude in 1891." Mr. Ranyard observed that if there were a resisting-medium a prominence would reach its full height sooner than *in vacuo.* Miss Clerke pointed out that the widening of the D_3 line, as observed by Mr. Evershed in the prominence of May 31st, was unprecedented.

The Rev. A. Freeman gave some notes on "Saturn's Ring-Shadow and Crape Ring in 1892."

Mr. Cottam read extracts from a paper by Mr. A. Stanley Williams on "Recent Observations of Bright Spots on Saturn." The writer's observations had led him to the conclusion that there were in all ten spots, of which eight had been observed on more than one night. Mr. Freeman called attention to the fact that spots were occasionally seen on the south side of the equatorial belt.

A paper on the "Present and Past Newtonian Telescope," by Mr. G. Calver, was also read.

The President announced that Mr. Wesley had undertaken the office of Librarian to the Society.

Mr. Maunder gave an account of his visit to Manchester and Liverpool, and stated that a strong branch would probably be formed in the former place. He then gave the first half of an interesting lecture on the spectroscope, illustrated by lantern-slides.

ROYAL METEOROLOGICAL SOCIETY.

The usual monthly meeting of this Society was held on Wednesday evening, the 17th February, at the Institution of Civil Engineers, 25 Great George Street, Westminster; Dr. C. Theodore Williams, President, in the Chair.

Capt. D. S. Cromarty, Mr. R. Godfrey, Assoc.M.Inst.C.E., Mr. C. Shapley, Mr. E. J. Smith, Mr. E. K. Speigelhalter, Rev. H. Stewart, and Rev. W. E. Stewart, M.A., were elected Fellows of the Society.

The following Papers were read:—

"The Untenability of an Atmospheric Hypothesis of Epidemics": by the Hon. Rollo Russell, M.A., F.R.Met.Soc. The author is of opinion that no kind of epidemic or plague is conveyed by the general atmosphere, but that all epidemics are caused by human conditions and communications capable of control. In this paper he investigates the manner of the propagation of influenza, and gives the dates of the outbreaks in 1890 at a large number of islands and other places in various parts of the world. Mr. Russell says that there is no definite or known atmospheric quality or movement on which the hypothesis of atmospheric conveyance can rest, and when closely approached it is found to be no more available than a phantom. Neither lower nor upper currents have ever taken a year to cross Europe from east to west, or adjusted their progress to the varying rate of human intercourse. Like other maladies of high infective capacity, influenza has spread most easily, other things being equal, in cold, calm weather, when ventilation in houses and railway cars is at a minimum, and when, perhaps, the breathing organs are most open to attack. But large and rapid communications seem to be of much more importance than mere climatic conditions. Across frozen and snow-covered countries and tropical regions it is conveyed at a speed corresponding, not with the movements of the atmosphere, but with the movements of population and merchandise. Its indifference to soil and air, apart from human habits depending on these, seems to eliminate all considerations of outside natural surroundings, and to leave only personal infectiveness, with all which this implies of subtle transmission, to account for its propagation.

"The Origin of Influenza Epidemics": by Mr. H. Harries, F.R.Met.Soc. The author has made an investigation into the facts connected with the great eruption of Krakatoa in 1883, and the atmospheric phenomena which were the direct outcome of that catastrophe. He has come to the conclusion that the dust derived from the interior of the Earth may be considered the principal factor concerned in the propagation of the recent influenza epidemics, and that, as this volcanic dust invaded the lower levels of the atmosphere, so a peculiar form of sickness assailed man and beast.

"Report on the Phenological Observations for 1891": by Mr. E. Mawley, F.R.Met.Soc. This report differs in many respects from the previous reports on the same subject. Among other changes the number of plants &c. selected for observation has been greatly reduced, while the number of observers has considerably increased. The winter of 1890–91 proved in England very destructive to the root-crops, as well as to green vegetables and tender shrubs. Birds also suffered severely. In Scotland and Ireland, however, there was scarcely any severe weather until March. The flowering of wild plants was greatly retarded by cold in the spring, but during the summer the departures from the average were not so great. The harvest was late and its ingathering much interfered with by stormy weather.

"Note on a Lightning Discharge at Thornbury, Gloucestershire, July 22nd, 1881": by Dr. E. H. Cook.

ASTRONOMICAL SOCIETY OF THE PACIFIC.

1892, January 30.

THE Meeting was held, by invitation, in the lecture-hall of the California Academy of Sciences, 819 Market Street, San Francisco. President Pierson presided. The minutes of the last Meeting were read and approved.

A list of thirty-eight members was read, who had been elected at the Directors' Meeting.

Among the presents received was a finely-coloured lithograph of the partially eclipsed Moon, drawn by Prof. Weinek, of Prague; and the Secretary announced that Prof. Weinek had presented 1000 copies to the Society for binding in the 'Publications.'

The following papers were presented:—

a. "The Rotation of the Sun" (translated from the German of Dr. Schmidt), by A. C. Behr, of Chicago.

b. "Pogson's Comet and the Bielan Meteors," by W. H. S. Monck, of Dublin, Ireland.

c. "The McKim Observatory," by Prof. W. V. Brown, of Greencastle, Indiana.

d. "When shall we have another Glacial Epoch?," by Garrett P. Serviss, of New York.

e. "The Total Eclipse of the Moon, January 28, 1888," by Prof. Weinek, of Prague (translated by F. R. Ziel, of San Francisco).

Committees to nominate a Board of Directors and Committee on Publication, and to audit the Treasurer's accounts, were appointed, to report at the Annual Meeting to be held March 26th.

The President announced that a Branch of the Society was being organized in Pittsburgh, similar to the Chicago Branch.

The room was then darkened, and seventy-five lantern-slides thrown on the screen, by Prof. W. W. Campbell, illustrating the methods employed and the results obtained in stellar photography and spectroscopy at the Lick and other Observatories.

Adjourned. CHARLES BURCKHALTER, *Secretary.*

THE NEW STAR IN AURIGA.

IN 'The Times' for Feb. 3, 1892, the following note appeared:—

"NEW STAR IN THE MILKY WAY.

"A new star has suddenly appeared in the constellation Auriga, in the Milky Way. It was searched for at the Royal Observatory, Greenwich, on Monday night, and in the position indicated a body answering to the description was found. This star, the existence of which was not shown in the charts or catalogues, differed somewhat in aspect from the other stars in the field, being slightly fuzzy, and its light not so piercing. A photograph was taken of the region, with five minutes' exposure. The star's position is approximately—Right Ascension $5^h\ 25^m\ 4^s$, and Declination $+30°\ 21'$, and its magnitude about $4\frac{1}{2}$, rather brighter than 26 Aurigæ, which it precedes by nearly two degrees. It is about two degrees south of χ Aurigæ. Astronomers will watch with interest the various stages in the development of the new phenomenon, and at Greenwich it is intended to follow its progress by a series of photographs with the new 13-in. photographic equatorial."

This note was the first intimation that most English observers received of the existence of the new star, but further information and observations were speedily forthcoming. The Observatory at Greenwich is unfortunately at the present time without a telescope of any considerable aperture, owing to preparations for mounting the 28-inch, which have necessitated the dismounting of the $12\frac{3}{4}$-inch, and consequently no spectroscopic observations could be made there.

The following circular, dated Feb. 2, was issued from the Royal Observatory, Edinburgh, and was generally received on Feb. 3:—

"Yesterday an anonymous postcard was received here bearing the following communication:—Nova in Auriga. In Milky Way, about two degrees south of χ Aurigæ, preceding 26 Aurigæ, 5th magnitude, slightly brighter than χ Aurigæ.'

"At $6^h\ 8^m$ Greenwich mean time the star was easily found with an opera-glass. It was of a yellow tint and of the 6th magnitude, being equal to 26 Aurigæ. Examined with a prism between the eye and the eyepiece of the 24-inch reflector it was immediately seen to possess a spectrum very like that of the Nova seen in 1866. The C line was intensely bright, a yellow line about D fairly visible, four bright lines or bands were conspicuous in the green, and lastly a bright line in the violet (probably Hγ) was easily seen. A telegraphic notice was sent to Greenwich in the afternoon, and later on, when the true nature of the object was recognized, to Kiel for general distribution. The star was photographed last night at Greenwich. Its place for 1892 is $5^h\ 25^m\ 3^s$, $+30°\ 21'$. It does not occur in the Bonn maps."

The telegram was of course sent to all subscribers to the 'Centralstelle für Astronomische Telegramme' at Kiel, but unfortunately very few English observatories subscribe to that organization.

Mr. Thomas D. Anderson, the author of the postcard to Prof. Copeland, is an amateur astronomer living in Edinburgh, and he discovered the new star by means of a star atlas and a small pocket telescope. In a letter to 'Nature,' Feb. 18, 1892 (p. 365), he announces that the star was visible as of the 5th magnitude certainly for two or three days, very probably a week, before the postcard was despatched to Prof. Copeland. It is almost certain that he observed the star on Jan. 24, and twice in the following week, but it was on each occasion taken for 26 Aurigæ. A comparison with the star atlas convinced him of the existence of the new star, and the postcard was accordingly written.

Observations of the new star were made on the evening of Feb. 3 at Dr. Common's observatory at Ealing with the 5-foot reflector. The star was of a yellowish-red colour, and was not noted as fuzzy, as it had been seen at Greenwich. The spectrum was found to be crowded with bright lines, many of them exceedingly vivid. C, F, and G lines of hydrogen were easily recognized. There was a bright line at or near D, one fairly bright between C and D (about 617), and several faint lines near both C and D. In the green between D and F three very strong bright lines were visible. One of these was found by comparison to be practically coincident with the 517 hydrocarbon fluting, while another was probably the 5005 nebula line. Several faint lines were observed in the green. Between G and F there were certainly three bright lines.

The spectrum of the Nova was photographed the same evening at South Kensington, and next day the following note was read to the Royal Society and published with the letter of Prof. Lockyer in the 'Times' of Feb. 5:—

"THE NEW STAR IN AURIGA.

"*To the Editor of the 'Times.'*

"SIR,—As the new star you announced in the 'Times' of yesterday may only remain brightly visible for a short time, it is of the highest importance to secure and compare observations of it during the first periods of its visibility.

"I have therefore received permission to forward to you a copy of a note I have read this afternoon at the Royal Society, in the hope that you will be good enough to allow me to use the wide and rapid circulation of 'The Times' in a matter where days, and perhaps hours, may be of importance.

"I am, Sir, your obedient servant,

"J. NORMAN LOCKYER,

"Ast.-Phys. Lab., Roy. Coll. Sci."

"Athenæum Club, Pall Mall, S.W.,

"Feb. 4."

"On the New Star in Auriga.—Preliminary Note by J. NORMAN LOCKYER, F.R.S.

"From a note in 'The Times' of Wednesday, February 3, I learnt that a new star had been discovered in the constellation Auriga, and that photographs had been obtained at Greenwich on Monday night.

"Observations were, therefore, impossible here before last night. This is much to be regretted, and suggests that some organization is needed to further quick transmission of news to observing stations relating to phenomena which may change in a few days, or even hours.

"Last night was fortunately fine, and two photographs were taken of the spectrum—the first exposed from 7.30–9, or for $1^h 30^m$; the second exposed from 9.30–12.30, or for $3^h 0^m$. The first registered 13 lines; the second appears to contain some additional ones, but they are very faint and have not yet been measured.

"A complete discussion of the photographs will form the substance of a subsequent communication, but already the following approximations to the wave-lengths have been obtained, the photographs being treated absolutely independently, means, however, being taken for the four least refrangible lines, as there has not yet been time to construct a proper curve for this region:—

Lines measured in the first photograph.			Lines measured in the second photograph.		
Wave-length.	Hydrogen lines.	Probable origin.	Wave-length.	Hydrogen lines.	Probable origin.
3933 K		Ca	3933 K		Ca
3968	H		3968	H	
4101	h		4101	h	
4128			4130		
4172			4172		
4226		Ca	4227		Ca
4268			4268		
4312		Hydro-carbon.	4310		Hydro-carbon.
4340	G		4340	G	
4516			4516		
4552			4552		
4587			4587		
4618			4618		

"I have recently taken up the question of stellar spectra and find that a 6-inch object-glass with a prism in front of it is all that is required for the brighter stars. This instrument was employed upon the Nova, which is of about the fifth magnitude, so the exposures were necessarily long.

"For the eye-observations the new 3-foot mirror which has recently been presented to the Astro-Physical Laboratory by Mr. Common was employed, but unfortunately the clock is not yet mounted, so that the observations were very difficult.

"C was the brightest line observed. In the green there were several lines, the brightest of which was in all probability F, the position being estimated by comparison with the flame of a wax taper. Another line was coincident—with the dispersion employed—with the radiation at wave-length 500 from burning magnesium wire. A fainter line between the two last-named was probably near λ 495, thus completing the trio of lines which is characteristic of the spectra of nebulæ. There was also a fairly bright line or band coincident with the edge of the carbon fluting at λ 517 given by the flame of the taper. A feeble line in the yellow was coincident under the conditions employed within the sodium line at D.

"The hydrogen line at G was distinctly seen, as well as a band, or group of lines, between G and F.

"Nearly all the lines appear to be approximately, if not actually, coincident with lines seen in the various types of Cygnus stars, the chief difference being the apparent existence of carbon, hydrocarbon, and calcium in the Nova.

"The colour was estimated by Mr. Fowler as reddish yellow, and by Mr. Baxendall as rather purplish. My own impression was that the star was reddish, with a purple tinge. This was in the 10-in. achromatic. In the 3-ft. reflector it was certainly less red than many stars of Group II. No nebulosity was observed either in the 3 ft. or the 10-in. refractor, nor does any appear in a photograph of the region taken by a 3½" Dallmeyer lens with three hours' exposure. It should be stated that the camera was carried by the photographic telescope, the clock of which had had its normal rate purposely changed to give breadth to the spectrum.

"The photographs were taken and reduced by Messrs. Fowler and Baxendall. The eye-observations and comparisons were made by Mr. Fowler."

"Astro-Physical Laboratory,
Royal College of Science, Feb. 4."

On Feb. 8th the Astronomer Royal wrote to 'The Times' as follows:—

"The following telegram has just been received through the 'Centralstelle für Astronomische Telegramme,' Kiel:—'Copeland's Nova bright on photograph Decr. 10, faint Decr. 1, maximum Decr. 20. Spectrum unique.—Pickering.' It would thus appear that Prof. Pickering had photographed the new star on the three dates named, in the course of the photographic mapping of stars and their spectra which he is carrying out at Harvard College Observatory, U. S. A., with the aid of funds provided as a memorial to Dr. H. Draper, and that thus a photographic record of the star has fortunately been obtained two months before its recognition as a new star."

It is not clear from Prof. Pickering's telegram if the new star was photographed as a star on the dates mentioned, or photographed as a spectrum in the ordinary course of the spectroscopic survey. Certainly neither the star nor its spectrum had been measured previous to the receipt of the telegram from Europe, and Prof. Pickering is forestalled in an important discovery because his large accumulation of photographs renders it almost impossible to examine them carefully immediately after they are taken.

There seems considerable doubt as to the magnitude of the star, which is variously estimated as from 4½ mag. to 6 mag., but as Prof. Pickering's photographs indicate a distinct variability these discrepancies are partially accounted for. After Feb. 5 the star was by pretty general agreement considered to be increasing in brightness, but the latest observations seem to indicate a diminution in brilliancy. Probably the next month will decide the question.

To the same paper Dr. Huggins also writes :—

"Sir,—The rapid increase of interest taken by the general public in astronomical events, and especially in phenomena so comparatively rare as the so-called 'new stars,' of which only about 18 have been recorded in as many centuries, induces me to ask you to give me space for a brief popular account of some of the results of our spectroscopic examination of the present Nova at this observatory, especially as a short note in your issue of yesterday *, unauthorized by me, may give an incorrect view of our observations. It is hardly necessary to say that we have no record of the addition of a permanently bright star to the emblazonry of the heavens. The new stars are of temporary splendour, and after a longer or shorter time, weeks or months, lay aside their suddenly acquired lustre and apparently return to the humble condition of very faint stars visible only in large telescopes. I am indebted to Prof. Copeland for a telegram received here shortly after midnight on the 1st instant. Our observations began on the evening of the 2nd, and when the star was not much brighter than the 5th mag., less bright than the star of 1866, which was about the 2nd mag., and that of 1876, which was about 3½ magnitude when first observed.

"The most noticeable feature in the new star's spectrum—

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* The note stated that Dr. Huggins, examining the spectrum of Nova Aurigæ, found the C and F lines strongly marked, and at first thought he saw a faint trace of the nebula line, which, however, further examination failed to establish.

common also to the stars of 1866 and 1876, and which Dr. Copeland had already seen—was the very great brilliancy of the red, green, and blue lines of Hydrogen. The reality of these lines was made certain by comparison with the corresponding lines of terrestrial Hydrogen. The bright double line of sodium was conspicuous and its nature confirmed by comparison with a flame containing sodium. Very striking were three brilliant lines in the green on the red side of F, and making with it a remarkably splendid quartet of bright lines. The first line is distant from F about one third of the interval separating it from the second line. The next line falls, as was shown by the method of direct comparison, very near the position of the chief nebular line. But it is not possible to say certainly as yet whether it is the nebular line or some other substance accidentally close to it. It is desirable that the motion in the line of sight of the star should be known, in consequence of which the whole spectrum might be shifted a little towards the red or blue. The cloudy state of the sky on Friday night prevented us from attempting the determination of the star's motion. The probability of the presence of the chief nebular line would be much greater if the second nebular line were found in the star, and there are indeed three lines, but much fainter relatively, not far from the position of the second nebular line. The third brilliant line from F appears in a small spectroscope to fall near the beginning of the brightest of the hydrocarbon flutings, but greater revolving power shows the line not to be coincident with the fluting.

"The spectrum of the star is superb, glittering with lines throughout. There are bright lines in the red near C, a brilliant line about one third of the distance from C to D, and there is a bright line on each side of D.

"If only the weather be propitious we hope to clear up the points I have mentioned as well as many others, and to supplement our eye observations by photographs of the invisible region of the spectrum.

"The Observatory,
Upper Tulse Hill, S.W.
Feb. 7."

"Your obedient Servant,
"WILLIAM HUGGINS."

In a note on the Spectrom of Nova Aurigæ, read at the Meeting of the Royal Society on Feb. 11, by Prof. Lockyer, he announces that the bright lines at K, H, *h*, and G are accompanied by dark lines on their more refrangible sides. He noticed that some of the lines, especially the bright one near F, on the less refrangible side, appeared to change rapidly in relative brightness. Careful comparisons showed the presence of the 5006 nebular line, the 517 magnesium, and the probable presence of 495. The 527 (E) line was also found, and the 557·6 fluting of manganese was sensibly coincident with another bright line. The photographs show 20 bright lines, and by direct comparison with the spectrum of α Cygni on the same plate the wave-lengths of these were measured. A striking agreement with the bright lines in nebulæ and bright-line stars is shown, and many of the dark lines in Orion stars are shown bright in the Nova. Several lines more refrangible than K are shown on the photograph. The lines are all broad, and the broadening is not accompanied by any falling off of intensity at the edges. This broadening may be due to atmospheric tremors, to the hazy appearance of the star as observed at Greenwich, or to internal commotions in the star itself. A slit spectroscope should decide this question, for the lines would be broad in that if internal agitations are disturbing the spectrum *.

Prof. Pickering, in his photographs taken at Harvard College, confirms the presence of the dark lines. The dark lines in the Nova are all on the more refrangible side, and do not vary from side to side as is found in the similar spectrum of β Lyræ. The relative motion in β Lyræ is approximately 300 miles per second, and in the new star 500 miles per second.

Dr. Vogel has photographed the spectrum of the Nova with the slit spectroscope at Potsdam, and finds that the terrestrial hydrogen lines fall between the bright lines and their dark companions in the Nova, indicating a motion of approach of the bright-line component and of recession of the dark-line component. The displacement is not equally divided between the components, but is greater for the bright lines than the dark ones.

In a communication to the Royal Society on Feb. 24 Dr. Huggins says:—

"Perhaps the most noticeable feature to the eye in the star's spectrum was the great brilliancy of the hydrogen lines at C, F, and G; but the point of greatest interest was obviously that two of these lines, F and G—and we have since observed the same with C—were accompanied each by a strong absorption line on the side towards the blue. Comparison with the lines of terrestrial hydrogen, while confirming the obvious presumption that the star lines were really those of hydrogen, showed at once a large motion of recession of the bright lines and a motion of approach of a similar order of magnitude of the hydrogen which produced the absorption.

"A photograph which we have since taken gives the star's spectrum as far in the ultra-violet as about λ 3200. On this plate we see not only the other hydrogen lines at *h* and H, but also the series beyond, which is characteristic of the white stars—bright, with dark absorption lines on the blue side.

"Besides the hydrogen series there appear to be other lines doubled in a similar manner, including the sodium lines at D. The line K is strongly impressed upon the plate, but in our photograph it is not followed by an absorption so strong as in the case of H.

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* With the narrowest possible slit the lines appear perfectly sharp in the star spectroscope on the 5-foot at Ealing.

"In the green part of the spectrum three very brilliant lines are seen on the red side of F. One of these falls not far from the position of the chief nebular line; but even when the shift of the spectrum is taken into account, we can scarcely regard this line as the true nebular line. In this connection it was a point of some importance to find that the strong and very characteristic line of the Orion nebula, which falls about λ 3725, is absent in our photograph of the Nova.

"The third line from F is rather broad and resolvable into lines. It falls partly upon the more refrangible pair of the magnesium triplet at *b*, but its character and position do not permit us to ascribe it to either magnesium or carbon.

"We wish to mention an early photograph of this star taken on the 3rd instant by Father Sidgreaves, at Stonyhurst, which we had the privilege of examining. This successful photograph extends from about *h* to near D, and shows the remarkable doubling of many of the bright lines by dark ones—a feature which was at once noticed by Father Sidgreaves and ourselves.

"In our photograph the spectrum of the star, which extends on the plate as far into the ultra-violet as our photographs of Sirius, is crowded throughout its entire length with dark and bright lines. In the visible region the number of bright lines and groups, including the double line of sodium and lines in the neighbourhood of C, is also very great.

"We prefer in this preliminary note not to enter into any more detailed discussion of the star's spectrum, nor to refer to the probable phenomena which may now be in progress in this celestial body. We reserve these considerations for the present."

More detailed observations will soon be available, for a vast amount of work is being done on the spectrum of the Nova. The arrival of the new star is very opportune, and it is hoped that the attention which is being paid to it will do much to clear up the controversies that have recently been rife as to the origin of these outbursts and their true meaning.

The Sun-spots, the Magnetic Storm, and the Aurora.

The abnormal condition of the Sun during the period 1892, Feb. 5–17, owing to the presence of a large group of sun-spots, attracted the attention of all solar observers. With such tremendous disturbances in progress in the solar photosphere, some disturbance of the magnetic condition of the Earth was to be expected, and the magnetic storm of Feb. 13–14 was characterized by an intensity quite in keeping with the solar disturbance with which it was connected. The magnetic storm, which seriously disturbed the telegraph and telephone services throughout the world, was attended by one of the most brilliant auroral displays of recent years.

The group of sun-spots to which these terrestrial disturbances are directly attributed appeared on the east limb of the Sun on 1892, Feb. 5, reached the central meridian on Feb. 11, and passed round the west limb on Feb. 17.

The total spotted area, measured on the photographs taken at Greenwich on Feb. 13, when the group reached its maximum, was no less than $\frac{1}{350}$ of the Sun's visible hemisphere. At Greenwich the area of spots is measured in millionths of the Sun's visible hemisphere, and this extensive group had an area of 2850 millionths, corresponding to 3360 millions of square miles. The centre of the group was then at 260° long., and in latitude $-23°$.

The group was a broad band extending over 22° of longitude in length and 10° of latitude in width, corresponding roughly to a greatest length of 150,000 miles and a width of 75,000 miles.

The large central spot of the group was 15° in length in longitude and 8° in width in latitude. The spot-group is the largest ever photographed at Greenwich, and is the largest which has appeared on the Sun since 1873. The large group of 1882, Nov. 18, was 2425 millionths of the Sun's visible hemisphere in area.

At Kew the magnetic disturbance commenced about 5.45 A.M. on Feb. 13, the easterly declination slightly increasing until about 5.40 P.M., while both horizontal and vertical forces increased in intensity, especially between 4 and 6 P.M. They diminished after 10 P.M., but the changes became very rapid from 12 o'clock midnight to 2 A.M. (Feb. 14), the declination proceeding to its extreme westerly position. The disturbance gradually diminished and died out about 4 P.M. Feb. 14. So rapid were the changes of force, and so great the extent of the vibrations of the free needles (over 2° in declination), that the Kew magnetometers could not record them.

At Potsdam the disturbance commenced on Feb. 13 about 6.30 P.M. (Berlin mean time), and enormous fluctuations of the needle were noticed. Changes of 2° in 2 minutes of time are recorded, and vibrations of over 3° were observed.

The records of the Greenwich magnetic instruments are fully dealt with in the notes of Mr. Ellis, published in this number.

The Aurora of 1892, Feb. 14, was observed at Greenwich at $0^h\ 35^m$ to $0^h\ 45^m$ as a brilliant patch of crimson light extending from the N.N.W. to N., with streamers of a whitish colour rising to an altitude of 50° or more, and converging to the right. Between $0^h\ 45^m$ and $0^h\ 50^m$ the crimson glow became more intense, afterwards dying away until at $0^h\ 55^m$ it disappeared, the streamers being visible to the last, but fluctuating in brightness. Light clouds formed to the North at $0^h\ 55^m$, and at $1^h\ 15^m$ the sky was completely overcast.

At Ealing the aurora was first noticed at $0^h\ 30^m$ as a faint crimson light in the N.N.W., which rapidly increased in intensity. Streamers of a yellowish-white colour were estimated to extend to an altitude of 60°, converging towards the north. Clouds immediately commenced to form to the N.N.W., and the contrast between the brilliant white clouds in the moonlight, the bright crimson sky, and the slightly yellowish streamers was very striking.

Attempts were made to observe the spectrum of the aurora with the star-spectroscopes used for the 5-foot reflector, but, except a faint brightening in the green, no details could be made out, owing to the bright moonlight.

The crimson glow at 0.50 A.M. extended round from N.N.W. to N.N.E., and a faint streamer arose from N.N.E. towards the north to an altitude of about 40°. The brightness of the streamers varied considerably, but they were visible until the sky was completely overcast at 1.10 A.M. The crimson glow was fainter after 1 A.M., but was visible through breaks until the clouds covered the sky.

Great Magnetic Disturbance of 1892, *February* 13–14.

THE magnetic disturbance commenced in all elements on February 13 at 5^h 30^m, Greenwich Civil Time, by a sudden increase of declination, horizontal force, and vertical force, accompanied by a strong manifestation of earth-currents. Large motions continued to be registered throughout the day and following night; between February 13 14^h and February 14 5^h they were unusually large, amounting in declination to 1° and more, the trace having passed off the sheet for one hour shortly after midnight. In horizontal force the disturbance exceeded 0·03 parts of the whole horizontal force, the trace having similarly passed off the sheet for nearly half an hour at about 22^h and for more than 1½ hours from shortly before 1^h to $2\frac{1}{2}^h$. In vertical force the disturbance was also great, the trace going off the sheet on both sides, in the direction of increasing force from $14\frac{1}{2}^h$ to 19^h, and in the direction of decreasing force from $0\frac{1}{2}^h$ to 2^h; the range probably exceeded 0·02 parts of the whole vertical force. The disturbance ceased on the afternoon of February 14. An aurora was seen at Greenwich between 0^h and 1^h on February 14.

A preliminary sudden movement is a common feature of magnetic storms; sometimes the disturbance follows on at once, sometimes it is a premonitory sign of disturbance to follow in a lesser or greater number of hours. The latter was the case in the celebrated 1859 sun-spot and magnetic disturbance. Mr. Ellis has recently been making an examination of some peculiarities connected with the initial movements observed in magnetic storms, and expects to arrive at interesting information in regard to the question as to how closely these movements are simultaneous at different places, and also on other points.

Selenographical Notes.

PICCOLOMINI.—This very notable ring-plain, which marks the westerly extension of the Altai Mountains, a range more terrestrial as regards its general characteristics than any other on the Moon's visible surface, requires more than a superficial examination in order that one striking structural peculiarity which it presents may be appreciated. Under a moderately high light its outline is defined by a more or less complete circular bright border, which answers to Webb's "noble ring," but towards sunrise or sunset the observer finds that about one-fifth of this bright circle is apparently deficient on the south-west. A reference to the maps throws no light on this curious defect, for they show no marked difference in the character of the border between this part of the circumference and the remainder.

If the formation be examined under morning illumination, when the west longitude of the terminator is about 28°, the observer, who has only seen it under a higher light or noted its regular boundary on the maps, will be somewhat taken aback on viewing the defect just mentioned, seemingly occupied by a triangular-shaped extension of the dusky exterior surface projecting far into the black shadow of the western wall. Under evening illumination, say when the west longitude of the terminator is about 40°, the defect is equally apparent, but at this phase a view is, of course, obtained of the whole of the wide inner slope of the western wall, and we see at once that it is not due to a want of continuity of the border, but to a very distinct difference between the reflective power of the material of which this south-western portion of the wall consists and the rest. There is, however, I think, another reason for the contrast in tone, which is suggested by appearances both at sunrise and sunset;—this south-west quadrant does not slope down to the floor in the usual way, at a more or less constant angle, but is broken up into a number of dome-shaped elevations, large and small, and exhibits no traces whatever of the regular concentric terraces so prominently displayed in every other part of the circumvallation. That these elevations, and the surface generally of this section of the wall, do not follow the gradient of the slope of the remainder of the west border, but tower above it, is clear from the fact that they are illuminated at sunrise, when the entire formation (excepting part of the central mountain) up to the foot of the summit ridges of the border is in black shadow. The division line running down the inner western slope between the dark quadrant and the light is very sharply defined under a low evening Sun and is well worth examination.

The bright plain north-west of the Altai range is shown by Schmidt to include a great number of craters and crater pits; but on February 3 (this year) 6^h to 7^h, I saw with a power of 284 very many more than appear in his map. I observed, however, no traces of the rill ϵ, or of its branch ζ. Though ϵ was discovered by Lohrmann in 1824, and is included by Schmidt in his

'Rillen' (S. 419), he, strangely, omits it in his chart. Neison describes it as "distinct," but I have only one record of it among many observations of the region. This plain extends as far north as Polybius and Fracastorius, and is remarkable not only for its brightness under most conditions of illumination, but as being one of the largest areas on the lunar surface devoid of conspicuous detail. Why (being level, or nearly so) it does not partake of the sombre colour of the Maria, may be left for explanation to those who believe that the dusky tone of these monotonous surfaces is due to the deposition of meteoric dust.

Fra Mauro B.—A letter has been received from Mr. R. Sprague, Berkeley, California, in which he records an observation, made on January 8, 1892, with a two-inch achromatic, furnished with a terrestrial eyepiece magnifying 64 times, of a crater which he believes is misplaced in Neison's Map. He subsequently ascertained that the object he saw was Fra Mauro B, in the position shown in B. and M.'s Map, which, he says, does not agree, as regards the place of the crater, with Neison's Map xiii. In this he is mistaken, as a careful examination of the two maps displays no discrepancy. In both the south latitude of Fra Mauro B is about $4° 5'$, and the east longitude about $21° 31'$. These co-ordinates agree very fairly with those of the same object in Schmidt's chart. Mr. Sprague also succeeded in detecting the little crater *f* south of Gambart with the same small telescope, which can hardly be such "an indifferent instrument" as he describes it.

Beaumont House, Shakespeare Road, T. Gwyn Elger.
Bedford, 1892, Feb. 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Jupiter.

Gentlemen,—

I read with interest the notes by Mr. Sells, in your January number, on the dark spots situated in the northern temperate belt of Jupiter, and agree with him as to the general appearance of these objects and as to the effects which variable definition has upon their aspect. But with regard to the rotation period of $9^h\ 57^m$ which he ascribes to them, it is much too great, and there is no doubt whatever that the spots he saw on Sept. 10, Oct. 8 and 18 were different markings. My own observations between Aug. 21 and Sept. 15 show that one of the chief spots gave a rotation-period of $9^h\ 49^m\ 27^s{\cdot}2$, while between Sept. 15 and Nov. 3 this had increased to $9^h\ 49^m\ 42^s{\cdot}2$, and these figures closely corroborate the retardation pointed out by Mr. A. Stanley Williams in your number for November, p. 361, and February, p. 111. For the whole period from Aug. 21 to Nov. 3 (comprising 180 rotations) the mean rotation was

$$9^h\ 49^m\ 36^s{\cdot}9.$$

This is $6^{m}\ 5^{s}$ shorter than the period derived from the red spot, so that the north temperate spots completed a revolution of Jupiter, relatively to the red, at intervals of 40½ days.

These mobile spots have presented all the phenomena which attended the remarkable outbreak of similar spots, on the same belt, in the autumn of 1880, and they possess an important bearing on the interesting question of periodical changes in the surface configuration of the planet. Both in 1880 and 1891 the southern part of a double belt, in about latitude +25°, exhibited active eruptions of dark material which rapidly distributed itself along the belt, increasing it most markedly both in thickness and in depth of tone. In the year 1870 the same belt appears to have been the seat of energy, for Mr. Gledhill observed dark condensations in it on Sept. 24, and in November and December of that year he noticed dark spots projecting from its southern side. It may therefore be confidently predicted that another outbreak of these variable spots will occur during the oppositions of Jupiter in 1901 or 1902, and that, as in 1880 and 1891, they will replenish the material of the northern temperate belt. The spots of 1891 certainly moved slower than those of 1880, which had a rotation-period of $9^{h}\ 48^{m}$. It would be interesting to consult reliable drawings of Jupiter made in 1869, 1858, &c., with an endeavour to trace whether similar disturbances occurred in the same latitude during those years.

Recent observations of the red spot have disclosed two noteworthy points in connection with this well-known marking. The spot has assumed a darker tone, so that it has been much plainer than during the few preceding years, though not nearly so pronounced as it was in the years 1878–1881, and its motion of rotation has considerably slackened. The following are the times of observed transits of the spot across the central meridian of the planet:—

Date.			Spot on C.M.	Longitude.
1891	August	7	$11^{h}\ 32^{m}$	1°·4
		21	13 2	1·4
		28	13 51	3·9
	September	12 ...	11 11	3·1
	December	11	5 43	5·0
1892	January	4	5 43	6·3
	February	2 ...	4 55	7·9

Thus during the six months the longitude increased 6°·5 ($=10^{m}\ 45^{s}$) relatively to the daily rate of 870°·27 ($=9^{h}\ 55^{m}\ 40^{s}\cdot63$) adopted by Mr. Marth in his Ephemeris, System II., 'Monthly Notices,' March 1891, p. 369 *et seq.* My observations as above indicate a mean period of

$$9^{h}\ 55^{m}\ 42^{s}\cdot2$$

from 432 rotations. This is 2^{s} in excess of that during the previous opposition of 1890, when the figures were $9^{h}\ 55^{m}\ 40^{s}\cdot2$, and it is 1^{s} greater than the period derived from the spot in any past opposition.

Yours faithfully,
W. F. Denning.

Bristol, 1892, Feb. 20.

OBSERVATORIES.

Harvard College.—Prof. Pickering still wants a fireproof room for the storage of photographs, and there seems some prospect of his getting a plain brick structure, for over 7000 dollars have been subscribed out of the 10,000 required. The photometric observations of Jupiter's satellites, the meridian work, and the work on variable stars of long period have progressed favourably. Three telescopes have been used continuously in connection with the Henry Draper Memorial, generally throughout every clear night. The nature and importance of this work has been frequently referred to. The use of a small prism attached to the objective for giving faint supplementary images of the brighter stars has been introduced with great success, and may be commended to the attention of other photographers. Mrs. Fleming's examination of the spectrum photographs has been very efficient, and has resulted in the detection of several new variables and a planetary nebula, and of nine new stars of the Wolf-Rayet type. Of the 37 Wolf-Rayet stars now known, 27 have been discovered at Harvard. The discovery of the changes in the spectrum of β Lyræ has been already referred to in detail (see No. 180, p. 341).

Mr. Bailey has returned from Peru with the meridian photometer and a good record of work done. Prof. W. H. Pickering and a 13-inch equatorial are at Arequipa, and since this instrument "appears to be the largest refracting telescope in use in the southern hemisphere, while about thirty larger telescopes are mounted in the northern hemisphere," there is obviously plenty of scope even for the energy of a Pickering.

The flint disks for the Bruce photographic telescope have been received, and the crown disks are expected shortly. "A photographic transit-instrument has been constructed, which is moved automatically and photographs on a single plate all stars brighter than the sixth magnitude which cross the meridian during three hours. With this instrument it is expected to prepare a catalogue like the Harvard Photometry, which will give the photographic magnitudes of the bright stars on a uniform scale. A second investigation now well advanced will give the brightness upon the same scale of about 40,000 stars of the tenth magnitude. These stars are distributed throughout the entire sky, so that one is in each square degree."

The Time Service is to be abandoned, according to a special circular. It is difficult to give a general notion of the state of publication. Volume xxxi. part 1 will soon appear, 130 pages being already in type, and yet Volume xv. is not complete and Volume xxv. is not yet undertaken.

Victoria.—We have received the twenty-sixth report of the Board of Visitors of this Observatory, in which is included the annual report of the Director, Mr. Ellery. The Board refers to the fact that the Astronomer and his principal assistants are approaching the age of retirement from active service, and recommend that inquiries should be made in England as to the possibility of obtaining the services of qualified persons from British observatories; and this paragraph we would commend to the attention of young professional astronomers. The Board refers among other matters to the desirability of converting the Great Melbourne Telescope into a refractor, and mentions that Sir Howard Grubb has been in communication with the Astronomer. The Board is now disposed to think that it would be better to have a separate instrument.

U.S. NAVAL.—"The new Naval Observatory buildings are not yet completed, and from present indications the removal from the old to the new observatory will not be accomplished until July 1892." Prof. Asaph Hall has been completing his double-star observations with the 26-inch equatorial before the instrument is removed to the new site, and a limited number of assistants have been diligent with the transit-circle and the 9·6-inch equatorial. With the latter the transit of Mercury on May 9 was observed; and 25 observers responded to a circular issued by the Observatory on that occasion. There is difficulty in getting funds for completing the reductions of the Transit of Venus (1882).

WOLSINGHAM.—Mr. Espin has discovered 120 new third-type stars, one fourth-type star, and five variable stars in the year 1891; and he has also much miscellaneous work to record in his annual report—meteorological, electrical, and otherwise. We congratulate him on the clearing off of the debt on the Observatory.

PUBLICATIONS.

ASTRONOMY AND ASTRO-PHYSICS. Edited by WM. W. PAYNE, Director of the Goodsell Observatory, Northfield, Minn., and GEORGE E. HALE, Director of the Kenwood Astro-Physical Observatory, Chicago, Ill. (Annual subscription 4 dollars; Published ten times a year.)—With the 101st number the 'Sidereal Messenger' has taken a new name, an additional editor, and a new departure. Prof. Hale, on the occasion of his recent visit to Europe, discussed the project of a journal specially devoted to Astro-Physics with most of the leading European spectroscopists, and found the idea was cordially received. Eminent spectroscopists in America were consulted and were equally favourable, and had it not been for the union with the 'Sidereal Messenger,' we should have had a separate Astro-Physical journal. As it is, we have an enlarged journal, very similar in general character to the 'Messenger.'

By its new departure the 'Sidereal Messenger' will attempt to place Astro-Physics in its proper place, side by side with and equal to the Astronomy of position. It will endeavour to assist as much as possible those investigators who "are not so fortunate as to have easy access to the files of all journals, the innumerable publications of learned societies, and the annals of all observatories." For this purpose translations and reprints of important papers will be freely used, and original contributions are promised from most of the great authorities on Astro-Physics.

The programme is an ambitious one, and in this first number is carried out with energy and ability that should command success. Indeed there seems a tendency to rather overdo the main idea, for while 16 pages are devoted to "General Astronomy" (one of the papers in this being, strictly speaking, a spectroscopic one), Astro-Physics occupy 70 pages, the usual "Current Celestial Phenomena" and "News and Notes" bringing the number up to 96 pages. All the articles are eminently readable and are of great value, the standard being almost uniformly excellent. To particularize is difficult under such circumstances.

Excellent photographs are introduced to illustrate a paper by F. H. Bigelow on a "Photographic Method of determining Star-transits," and a paper by George E. Hale on "Recent Results in Solar prominence Photography." Plate III. (to illustrate the latter paper) is specially remarkable, as showing a wonderful agreement between photographs of a solar prominence taken at Chicago on 1891, July 9, at 11.55 local mean time, and a drawing of the same prominence made at the same time by Herr Julius Fényi, S.J., Director of the Haynald Observatory at Kalocsa, Hungary. The general form is the same, and the estimated height, 80,000 miles, is in excellent agreement.

The 'Messenger' in its new form has certainly entered upon a career of increased usefulness, and has become essential to all workers in Astro-Physics, while it will not, it is hoped, lose any of its interest or value to workers in other departments of Astronomy. We hope it will receive proper support from the growing army of spectroscopists, and wish it all success.

THE MILKY WAY FROM THE NORTH POLE TO 10° OF SOUTH DECLINATION. Drawn at the Earl of Rosse's Observatory at Birr Castle by OTTO BOEDDICKER. (Longman, Green & Co., price 30*s.*)—This work consists of four beautifully executed lithographic reproductions of the drawing of the Milky Way as it appears to the naked eye to Dr. Boeddicker, which he presented to the Royal Astronomical Society in November 1889, together with a brief description of the plates by the observer.

Plate I. is a detailed drawing of the Section Cygnus—Scutum, Plate II. of the Section Cassiopeia, and Plate III. of the Section Aurigæ—Gemini—Monocerotis. In these plates an attempt has been made to represent the differences of luminosity as they actually appeared, and careful notes are given in the introductory preface pointing out where the artist believes that the pictures are imperfect in this respect. Plate IV. is a general view on a smaller scale, with the whole of the detail deliberately exaggerated. The highest praise is due to Mr. Wesley for the care and skill he has bestowed upon the lithographic reproduction of these drawings of the Milky Way.

Dr. Boeddicker is to be congratulated upon the completion of his long and arduous task, which he carried out in a thoroughly conscientious manner, and upon the excellent form in which the results of his work are published. He believes that "the first step necessary towards a knowledge of the sidereal universe is a thorough acquaintance with the Milky Way as it appears to the naked eye," and has devoted the five years from Oct. 1884 to 1889 in making a drawing of it as it appeared to him. It is remarkable that an observer having an opportunity of using the telescope with the largest aperture in the world, should have used the clear moonless nights, *i. e.*, those most favourable for observations of details in nebulæ, in making a *naked-eye* picture of the Milky Way. He seems to pride himself upon the fact that he has not used optical aid of any kind, but he omits to give any details of the peculiarities of his own eyes. It is a well-known fact that it is practically impossible to find any two pairs of eyes exactly alike, and, knowing this, the stress laid upon the fact that unaided eyes were used throughout seems incomprehensible. A picture made with slight optical aid, such, for instance, as a pair of opera-glasses could have afforded, would have been more likely to represent the actual structure of the Milky Way than any naked-eye drawing, and it would have been possible for other observers to check the drawing, for the opera-glasses to some extent correct visual peculiarities. All peculiarities depending upon long or short sight would undoubtedly have been removed. As it is, we cannot help believing that some of the wisps and feelers introduced in these drawings are due to glare of stars or irradiation in the retinæ of Dr. Boeddicker's eyes, and probably no other pair of eyes would see them in exactly the same manner or to the same extent as he does.

The photographs of the Milky Way by Barnard, Russell, and Wolf undoubtedly do not represent the Milky Way as it appears to the naked eye, since most of the stars on them are quite beyond the range of visibility. We scarcely follow the statement that "by superimposing the (naked-eye) drawing on the photographs some knowledge of the structure of the Milky Way in the line of sight may be obtained." There does not seem any reason for supposing that, because certain stars give an appearance of luminosity to the sky, while other stars are only detected by long exposures, therefore the former stars are nearer the Earth than the latter. All the stars in the Milky Way are not of the same actual size, and their apparent visual magnitudes, or their photographic magnitudes, are not measures of, or in any way directly connected with, their distances. Unless this is so, it is difficult to see how we are to get any definite idea of structure in the line of sight.

There can be no doubt that for the purpose of studying the structure of the sidereal universe the final resort must be photography, and by a series of photographs of the Milky Way, with different exposures, some idea can be formed of the distribution of the stars according to photographic magnitude; but the value of naked-eye pictures is very small, and is difficult to estimate unless we have full details of the peculiarities of the eyes of the observer.

It is to be regretted that the Milky Way is described as "nebulous matter" and "nebulosity" in many places in the preface. These terms have definite meanings, and are not synonyms for luminous matter and luminosity, the sense in which they are used here. The distinction between a nebula and a cluster is a rigid spectroscopic one, and cannot be insisted upon too forcibly, and light from a large cluster, such as the Milky Way, is not "nebulous" light or "nebulosity," and the matter is not "nebulous matter."

The work is an interesting addition to astronomical charts, and will be exceedingly useful to all observers who wish to carefully study the Milky Way. The labour involved in its production was enormous, and the detail shown far exceeds that indicated in any previous drawings of the Milky Way.

ASTRONOMICAL SOCIETY OF THE PACIFIC.—Volume iii. No. 19 (1891) opens with Dr. Weinek's article on the Lick photographs of the Moon, which were submitted to him for drawing; and very beautiful drawings he has made from them, to judge by the frontispiece. From a note by Prof. Holden we learn that the drawings are to be reproduced by *heliogravure*, which is found adequate and entirely satisfactory. "Some specimens have already been received, and they show conclusively that the question of reproducing such work is solved."

Two papers by Messrs. Schaeberle and Campbell contain evidence that Jupiter's first satellite is elongated and the others practically round; and that the first and third rotate on their axes once in a revolution round the planet. Several drawings of markings on the third satellite are reproduced.

Volume iv. No. 20 (1892) is a list of Members of the Society.

THE DISTRIBUTION OF THE MOON'S HEAT AND ITS VARIATION WITH THE PHASE.—The Utrecht Society of Arts and Sciences proposed as a subject for their prize essay in the year 1890 the question of the Moon's radiant heat in different phases of the Moon; Mr. Frank W. Very, who has been associated with Prof. Langley in similar researches, was awarded the prize for the investigation which now lies before us.

The apparatus with which the observations were made, and which may be briefly described as a siderostat and bolometer, was put at Mr. Very's disposal by the authorities of the Allegheny Observatory, where the observations were made. This apparatus might be used in either of two ways; in one the whole lunar image would be projected on the sensitive surface of the bolometer, in the other particular regions of the Moon would be made in turn to influence the bolometer. Mr. Very has adopted the latter method. The lunar image having been projected on a white card pierced with a small hole, the image of the region whose heat was to be measured was brought over this hole, through which the rays passed to the sensitive surface of the heat measurer. The results of this method are given in a series of 8 charts of the Moon in different phases, each chart representing a night's observation, for Mr. Very has elected to give a few nights of good observation rather than many of inferior quality. On these charts are marked the different spots whose heat has been observed, corresponding tables give the measures, and isothermal lines inferred from these measures are drawn on the charts. We thus have a convenient means of studying the varying radiation of any individual region under different conditions of Sun's altitude. For instance, it may be noted that on the part of the Moon to which the Sun is setting the heat-gradient appears steeper than on that part to which the Sun is rising.

Finally, by dividing the Moon's surface into elements and effecting a summation of the corresponding "heat-volumes," the total heat of the Moon in its different phases is found. These results are laid down and a curve drawn which agrees fairly with the similar one found by Lord Rosse, although it may be objected to the present curve that it depends on too few observations. H. P. H.

NOTES.

The Mid-February Cold.—After the severe winter of 1890–1891, we had hoped to somewhat escape any similar visitation during the present winter, and the rather genial weather experienced early in February seemed to give promise that we might consider that we were getting well through; but this was not to be. On February 12 a cold period set in, lasting till February 20, and on some days during this period the cold was very severe, accompanied as it was by snow-storms and bleak winds. The lowest temperatures observed at Greenwich from February 16 to 19 were 23°·2, 18°·8, 21°·5, and 21°·1 respectively. During the period since 1841 temperatures lower than 18°·8 have been experienced on these four days only in the year 1855, in which year, on Feb. 17, the lowest was 18°·0, on Feb. 18, 13°·1, and on Feb. 19, 11°·1. The mean temperature of the four days in this year was 27°·6, 26°·7, 28°·1, and 28°·9, respectively, below the average of 20 years by 11°·2, 12°·2, 10°·9, and 10°·3. Lower mean temperatures than 26°·7 on these days, since 1841, have been observed also only in 1855, when that of Feb. 16 was 23°·8, Feb. 17, 21°·4, Feb. 18, 20°·9, and Feb. 19, 22°·0. In regard to the lowest temperature, 18°·8, observed at Greenwich during the four days in question, it is to be remarked that a very much lower temperature appears to have been recorded at other places in England.

Comet Notes.—We continue the ephemeris of Wolf's Comet, though it is now too faint for any ordinary instrument:—

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
Mar. 1....	5	0	15	4	55	Mar. 21....	5	27	1	2	8
5....		5	19	4	19	25....		32	43	1	40
9....		10	33	3	44	29....		38	30	1	13
13....		15	55	3	11	Apr. 2....		44	22	0	47
17....		21	25	2	39						

Brightness 0·89 on Mar. 2; 0·49 on Apr. 3.

Gautier publishes (Ast. Nach. 3075) the following search ephemeris of Tempel's Comet (1867 II.):—

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
Mar. 1....	17	6	53	18	10	Mar. 21....	17	39	53	19	44
6....		15	37	18	34	26....		47	11	20	7
11....		24	3	18	58	31....		54	1	20	30
16....		32	9	19	22	Apr. 5....	18	0	20	20	54

There is a probable error of some 16^m in the R.A., and 1¼° in decl.

We continue the search ephemeris of Winnecke's Comet, from Ast. Nach. 3062:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
Mar. 1....	12	52	36	23	48	Mar. 21....	12	40	27	31	55
5....		51	34	25	17	25....		35	49	33	37
9....		49	52	26	52	29....		30	28	35	17
13....		47	28	28	30	Apr. 2....		24	27	36	53
17....		44	20	30	12	6....		17	52	38	23

The southern part of Ophiuchus, Serpens, the southern part of Boötes, and Coma Berenices may be swept for Brooks' Comet (1886 IV.). But the orbit is too uncertain to make a published ephemeris of much value.

THE Lowndean Professorship, vacant by the death of Prof. Adams, was filled on Saturday, February 19, by the election of Sir Robert Stawell Ball, F.R.S., St. Andrew's Professor of Astronomy at Trinity College, Dublin. The electors were the Vice-Chancellor (Dr. J. F. Peile), the President of the Royal Society (Lord Kelvin), the President of the Astronomical Society (Mr. Knobel is not in England at present), the Astronomer Royal, and the Lucasian, Plumian, and Sadlerian Professors (Sir G. G. Stokes, Mr. G. H. Darwin, and Mr. A. Cayley). The appointment does not necessarily include the Directorship of the Cambridge Observatory, which was assigned, by grace of the Senate, to the Plumian Professor in 1822, and held accordingly by Woodhouse (1822 to 1828), Airy (1828 to 1835), and Challis (1836 to 1861). In 1861 Prof. Challis wished to write a treatise on Hydrodynamics, and asked to be relieved from the care of the Observatory. As a temporary arrangement, therefore, Prof. Adams, the Lowndean Professor, undertook the directorship; and this arrangement was not revised in 1883 when Mr. Darwin was elected Plumian Professor in succession to Mr. Challis. There is thus some uncertainty as to the directorship, which will be decided by the Observatory Syndicate.

The Professorship is for Astronomy and Geometry; and Sir Robert Ball is thus peculiarly well fitted for the position; for not only has he a well-deserved reputation as an astronomer, but he has developed a very complete geometrical theory which he has christened the "Theory of Screws." In heartily congratulating him upon his appointment, we may perhaps express a wish that the Observatory will be ultimately assigned to him and will claim a just share of his attention. Since Airy left it in 1835 the Cambridge Observatory has not taken its proper place in the astronomical world.

A VERY clearly written article by Mr. J. E. Keeler on the "Elementary Principles governing the Efficiency of Spectroscopes for Astronomical Purposes" has been reprinted from the 'Sidereal Messenger' (No. 99), and it is worth a little trouble to acquire a copy for reference. Such principles, although elementary, are sometimes forgotten or even misapprehended by those who might be expected to keep them prominently in view, as Mr. Keeler very justly observes; and the paper also refers to some things not generally known, such as Lord Rayleigh's recent remarks on the resolving power of a spectroscope (see 'Observatory,' No. 179, p. 294).

No. 6 of Vol. x. of the Transactions of the New York Academy of Sciences contains an index to an astronomical journal commenced in 1881 by Dr. Ralph Copeland and Dr. J. L. E. Dreyer, called 'Urania,' a name given up for outside reasons and replaced by that of 'Copernicus.' This index furnishes a means of reference to many valuable papers that would be otherwise overlooked, as day by day the difficulty of tracing papers on special branches becomes greater even with the regular publications. We are glad that this work has been done.

WE regret to hear of the death of Mr. Joseph Kleiber, on February 12, at Nice. He was ill for nine months preceding his death. Originally an astronomer, he was attracted to Cambridge some few years back by what he had read of Cambridge mathematics; and his work then had such fascinations for him that it was only by an effort that he returned to astronomy. While in England he was elected a Fellow of the R. A. S. on 1889 December 13, and did a good piece of work in determining parabolic orbits for the meteor-streams catalogued by Mr. Denning ('Monthly Notices,' Vol. li. p. 407).

ERRATA in the February Number of the 'Observatory.'

P. 125: Mean Temperature 1861, *for* 49°·2 *read* 49°·8.
" " " 1889, " 48 ·0 " 48 ·8.
" Departure from average 1856, *for* −0°·3 *read* −0°·2.
" Mean Temperature for 40 years, 2nd Quarter, *for* 53°·0 *read* 53°·1.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 187. APRIL. 1892.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, March 11, 1892.

E. J. Stone, M.A., F.R.S., *Vice-President* (Past President), in the Chair.

Secretaries: H. H. Turner, M.A., B.Sc., and E. W. Maunder.

The Minutes of the previous Meeting were read and confirmed.

Mr. Maunder. One hundred and twelve presents have been received since the date of the last Meeting. Those calling for special mention are:—'Comparative Photographic Spectra of the Sun and Metals, and Comparative Photographic Spectra of High and Low Sun,' presented by F. McClean, Esq.; Otto Boeddicker—'The Milky Way from the North Pole to 10° South Declination, drawn at the Earl of Rosse's Observatory, Birr Castle,' presented by the Earl of Rosse; Photographs of the Spectroscope at the Kenwood Astro-Physical Observatory, Chicago, presented by Prof. G. E. Hale.

A vote of thanks was passed to the donors of the presents.

Mr. Turner read an abstract of a paper on "A New Binary Star, β 581," by *Mr. S. W. Burnham.* Mr. Burnham discovered in 1878 this fine triple star, the interest in which consisted of the fact that it was almost a replica of Zeta Cancri. Later measures have shown it is a binary revolving, according to Mr. Burnham, at a rate of 360° in 58 years, which is almost exactly the period of ζ Cancri. With this star, however, the movement is direct, whereas with ζ Cancri the revolution is retrograde.

Mr. Burnham has also contributed another paper, entitled "Observations of Nebulæ with the 36-inch refractor of the Lick Observatory." This paper represents a great deal of laborious observation and computation. The observations were made in the months of September, October, and November, 1891, while other work was being proceeded with, but so as not to interfere with the micrometer measures of close double stars. A large number of nebulæ from Dr. Dreyer's new general catalogue have been examined, and as a result of three months' work we have 50 pages of manuscript, each page containing several observations. Mr. Burnham is impressed with the necessity of determining the positions of nebulæ with accuracy, and has been measuring them with a micrometer and noting distances from and positions with regard to stars, thus laying up a store of material for determining any changes in the nebulæ.

A vote of thanks was accorded to Mr. Burnham for his papers.

Mr. Turner. The next paper is one with a melancholy interest. The Secretaries have received a letter from Mr. Kleiber, brother of one of the Fellows of the Society who has recently died, stating that the late Dr. J. Kleiber has left behind him a paper entitled "Displacements of Apparent Radiant Points of Meteor Showers, due to the Attraction, Rotation, and Orbital Motion of the Earth." The last wish of Dr. Kleiber was that this paper should be sent to the Society, but as he was ill for nine months preceding his death, it might not be all that he desired it to be. The paper is of considerable interest, being an able discussion of the points mentioned in the title. He had previously taken the careful observations of Mr. Denning, and had reduced them so as to obtain parabolic orbits from the radiant-points recorded.

In the present paper the theories of the Perseids and Andromedes are fully developed, and the differences between the two cases are carefully brought out.

The paper does not lend itself very well to verbatim reading. The subject is one which has occupied the attention of several of our Fellows in recent months, and this discussion of the effects of orbital motion and of rotation on the position of the radiant will be extremely useful. With regard to the change of radiant of the Perseid swarm, Dr. Kleiber says that the orbital motion of the Earth will explain the apparent shifting of the radiant with even greater accuracy than might have been expected.

The Chairman. A great number of our Fellows knew Dr. Kleiber, and we shall all feel the loss we have sustained in his death. He spent many years amongst us in England, particularly at Cambridge, improving his mathematical reading, and afterwards turned the knowledge thus gained to the service of Astronomy. We must all regret that early death has snatched him from us.

Mr. Turner. We have received several papers on the magnitude of the new star in Auriga, and Mr. Wesley has prepared a diagram showing the magnitudes determined by different observers. Those from Greenwich are photographic determinations, and vary from 3·5 to 6·5. Prof. Pritchard's are made with the wedge photometer and by photography, and are consistently different from the Greenwich photographic results. Mr. Knott's agree with Prof. Pritchard's determinations, and are from eye comparisons with stars in the Oxford catalogue. This will probably account for their agreement with the Oxford results, and their differences from the Greenwich magnitudes. Mr. Roberts' determinations are photographic, and while the Greenwich photographs and the eye observations of Pritchard and Knott show considerable variations, Mr. Roberts' results show no appreciable variation at all.

The Astronomer Royal read his paper entitled "Photographic Magnitude of *Nova Aurigæ* as determined at the Royal Observatory, Greenwich."

Unfortunately the cloudy weather has interfered with the continuity of the series of observations at Greenwich, and the lines connecting the observed points on the diagram which Mr. Turner has had prepared must not therefore be taken as defining the magnitude of the star between the dates of observation. Another general remark I will make is, that the photographic brightness of the star is quite a different thing from the visual brightness; therefore we must not be surprised to find that the star appears in the photographic magnitude at one time to be brighter than in the visual magnitude, while at another time it may be equal to it or fainter than it. If the star gets fainter during its history, as a new star commonly does, it will probably become red, and consequently photographically faint. I have not brought photographs to show this star, because they would only have been similar to the photographs I exhibited at the January meeting of the Society. The discovery of this star was communicated in the first instance by Dr. Copeland, who announced that an anonymous post-card had informed him that a new star had appeared. After we received the information at Greenwich, photographs were taken with exposures ranging from 12 minutes to 15 seconds. For one plate we gave an exposure of 12 minutes, of 30 seconds, and of 15 seconds; for another plate an exposure of 4 minutes, 2 minutes, and 1 minute; and for another plate 8 minutes and 6 minutes, so that we had exposures of 12 minutes, 8 minutes, 6 minutes, 4 minutes, 2 minutes, 30 seconds, and 15 seconds. That was done with the object of getting images of the new star which would not be too large to be fairly measurable and comparable with images of the fainter stars of the 8th or 9th mag., which presumably had been fairly well determined as to brightness by Argelander, and which will be comparable with the new star as it diminishes in brightness and becomes gradually extinct. The range from 12 minutes to 15 seconds would represent 4·2 magnitudes, so, supposing the star was of the fourth mag., the image of the new star with 15 seconds' exposure would be comparable in diameter with the image of the comparison star of 8·2 mag. of 12 minutes, and the intermediate exposures would fill up the series. This therefore gave us not only the means of determining the mag. of the star, but of testing the law which had been adopted for connecting size of image with exposure; that is to say, that the square root of the diameter varies as the logarithm of the time of exposure. The measures of the photographs have been made by two of the lady Astronomers engaged at the Observatory, Miss Everett and Miss Rix, and by myself. On comparing results, it appears that there is a certain amount of personality, which is not to be wondered at in estimating the diameter of the diffused disc with a penumbral fringe.

Measures were made on these plates of 4 comparison stars between 8 and 9 mag., and also of another comparison star of 5·7 mag., as estimated photometrically at Oxford and Harvard. From those measures the value of the constant for the plate was deduced from each exposure, and an examination of that shows that there is no systematic difference between the long exposure of 12^m and the short exposures of 30^s and 15^s representing a range of four magnitudes. That was an independent verification of the truth of the law as applying to the plates. Then, knowing the constant, and measuring the diameter of the Nova, the mag. of the Nova was deduced on each plate, and the mean taken for the day. On comparing these results, it was evident that the amount of personality between the two observers who made most of the measures—Miss Everett and Miss Rix—amounted to three-tenths of a mag. on the average. There were certain discordances which we must expect, and I am afraid that this being a new subject we have to feel our way very much. I have thought it would be safest to take the mean of the results, and these Mr. Turner has had copied on the diagram. They are formed by comparing the Nova, not only with the four stars between 8 and 9 mag., but also with a star of 5·7 mag. In the course of the work it appeared that this star was photographically much brighter than the visual mag. would indicate, the difference being ·66, or two-thirds of a mag. Looking to the variations which have been remarked between photographic and visual mag., that is nothing to be surprised at, and in order to reduce the magnitudes all to one system, I have corrected the value of the mag. deduced from this star by that quantity, ·66, so they all refer to the four Argelander stars of 8–9 mag. It will be observed, therefore, that the mag. thus deduced rests entirely on the photographic mag. of these 4 stars. If these stars should be faint photographically, the results given show the Nova in Aurigæ brighter than it should be.

There is one very striking point in connexion with the observations, and that is that the star increased in brightness from Feb. 1 (the first observation) to a maximum on Feb. 3. It was found to have diminished in brightness on Feb. 12, and it fell in the next photograph on Feb. 13; but on Feb. 18 it was found to be decidedly brighter again, having brightened to a maximum nearly equal to that on Feb. 3, and from that time it has diminished. The record is very much interrupted, and we cannot infer anything as to what occurred between the dates of observation. The general result of the discussion of the Greenwich photographs is that there was a primary maximum, so far as the record goes, on Feb. 3 or thereabouts, and a secondary maximum about Feb. 18. Since that time it appears to have diminished, and now it is diminishing in brightness. The last measure I have of the brightness of the star gives about 6th mag. on March 9. The mags. deduced are:—Feb. 1, 4·5; Feb. 3, 3·5; Feb. 13, 4·5; Feb. 18, 3·8, and after that it has diminished to the 6th mag. These results which I have brought before the Meeting must be considered as to some extent provisional, this being the first experience we have had in deducing photographic mag. from the measures of photographs. I have, however, considerable confidence in the results, because they have been made by three independent observers, and checked as far as we could.

Father Sidgreaves. The photographic impressions of the spectrum of the new star taken at Stonyhurst fully confirm what the Astronomer Royal has said about the increase of photographic magnitude on Feb. 18. I have 10 plates of the spectrum taken on different nights between Feb. 3 and March 5. They are mostly feeble impressions; but that of Feb. 3 is strong, and the plate of the 18th shows nearly equal actinic energy, if we take into account the less favourable condition of the sky. The impression produced upon me by the series of plates is that the radiation energy of the principal bright lines would represent still higher magnitudes than those of the Greenwich curve in a continuous spectrum of the same intensity.

Mr. Ranyard. There is one point with regard to those three curves which is worthy of notice. In spite of the serious differences as to the magnitude of this Nova, all of the three curves show a secondary maximum on the descending branch, and this is also the case in the light curves of very many variable stars. As far as this evidence goes, it indicates that this star obeys the same law of decrease as the long-period variables, and that this star is probably a body of the same type as the variables.

Prof. Lockyer. Before giving an account of our spectrum photographs of the new star in Auriga, I should like to express my regret that we have not some *local* organisation for the distribution of information. The present arrangement is not by any means satisfactory, for a message must be sent from, say, Greenwich to Kiel, and then from Kiel to South Kensington or any other observatory in England. This is not what should be the case, and I for one should be very pleased if the Society could arrange some local centre for the rapid distribution of news of all discoveries of this kind *.

At South Kensington the first news we got of the discovery was from 'The Times' of Feb. 3. Good photographs of the spectrum were obtained that night by the aid of the 6-inch photographic telescope with a 6-inch prism in front of the object-glass. Good photographs of the spectrum have also been obtained in this manner on Feb. 3, 7, 13, and 22, the photographs taken on other days being generally poor owing to bad weather and other causes. The photograph of Feb. 3 showed 11 bright lines in the spectrum, and on that the dark companions to the lines were suspected. It was on Feb. 7 that we obtained unmistakable evidence that many of the bright lines were accompanied on their more refrangible sides by corresponding dark lines, and this result was announced to the Royal Society on Feb. 8. [A lantern-slide of the spectrum of the Nova photographed at South Kensington on Feb. 7 was projected on the screen.] The relative velocity indicated on all the photographs taken is about 600 miles per second.

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* [It is proposed to bring this matter before the Council of the Society.—Eds.]

The bright hydrogen lines are broad in the photograph, although under exactly the same instrumental conditions other stars gave perfectly sharp lines.

[A slide was shown on which the spectrum of the Nova was seen with the Hydrogen lines broad, while Arcturus on another part of the same plate showed perfectly sharp definition. Prof. Lockyer also showed a table of the wave-lengths measured in the spectrum of the Nova, together with bright lines in the spectra of the Wolf-Rayet stars as photographed by Prof. Pickering, the dark lines photographed in the Orion Stars at South Kensington, and the spectrum of the Nebula in Orion photographed at Westgate. The lines in the Nova were found to be mostly coincident with lines in these spectra. The photographic evidence so far supports the collision theory of the constitution of new stars, and this view is endorsed by Prof. Pickering.]

Mr. Knott. My estimations of the magnitude of the new star were made with a binocular field-glass of magnifying-power 5, the comparison stars being χ and 26 Aurigæ and L 10143 from Pritchard's 'Uranometria Nova Oxoniensis.' The slight fluctuations of magnitude observed may perhaps be partly due to errors of observation. From Feb. 3 to 12 the star was 5·1; it then diminished to 5·9 on Feb. 22 and 23, rose to 5·6 on Feb. 25 and 27, 5·5 on Feb. 28, and since then has steadily diminished to 6·1 on March 7. The approximate mean R.A. of the star was found on Feb. 20 to be $5^h\ 25^m\ 3^s\!\cdot\!33$. The colour was noted as pale yellow, or pale yellow with a slight tinge of orange. The spectrum was observed with a small direct-vision spectroscope fitting over the field-lens of an ordinary negative eyepiece of magnifying-power 200. The lines seen accorded with those observed by Dr. Copeland at Edinburgh.

Mr. Roberts. I photographed the region of the new star, with 45 minutes' exposure, on Feb. 5, the first clear night after the receipt of the Edinburgh Circular, No. 22, announcing the discovery. Two photographs, of 5 min. and 15 min., were taken on Feb. 13, and one with 3 hours on Feb. 18, an enlargement of which is now presented to the Society. Two others were obtained on Feb. 22, and two on Feb. 25. The margin of the star-image on the photographs is sometimes well-defined and sometimes nebulous and ill-defined. The diameter of the star-image was measured with the pantagraver to ·0001 of an inch, and the diameter on each plate, together with those of D.M. +30° No. 899 and 26 Aurigæ, are given in the table. In no instance do the photographic images of the Nova and the comparison stars differ by a quantity exceeding 4 secs. of arc in a diameter of 30 secs.

The Astronomer Royal. The star Mr. Roberts used as a comparison star was the one used at Greenwich, but we took the magnitude from photometric measures, and not from Argelander, as Mr. Roberts has done; there is therefore a difference of half a magnitude in the assumed magnitude of the comparison star. Mr. Roberts says that in his results there was not a difference of more than 4″ in a diameter of 30″. This corresponds to a whole magnitude, and is a considerable quantity when we are dealing with a variation of magnitude, as in this case. As Mr. Roberts has said, there is really nothing contradictory in his observations to those which have been made elsewhere.

Mr. Maunder. The occurrence of this star came at a very inopportune moment so far as spectroscopic work at Greenwich is concerned, since the old 12-in. refractor had been dismounted and the new 28-in. had not come to hand. There was no telescope in the Observatory to which either of our spectroscopes could be applied. Under these circumstances the Astronomer Royal directed me to use an object-glass prism which Sir Henry Thompson presented to the Observatory some time ago with the 9-in. photographic telescope. The 9-in. photo. telescope had, however, been adapted for taking the solar photographs, and the prism had not been used in conjunction with it; so when we set to work to fit the prism to the telescope several difficulties were encountered, and it was not until Feb. 18 that I was able to make the first exposure on the Nova. That was a failure, because the sky clouded before the exposure was completed; but on Feb. 22 the night was fine, and I got the only photograph of the Nova which we have secured. The spectrum is by no means a sharp one, for the tube of the telescope was too short to get a sharp focus, and the telescope being mounted on the same mounting as the Lassell reflector, the great weight of the latter caused considerable flexure when the telescope was away from the meridian, and it was found very difficult to keep the star in its place in declination. The photograph, however, shows the principal lines to which Mr. Lockyer has drawn attention. I was able to measure six dark and fifteen bright lines—the bright lines were principally those of hydrogen, of which all the lines were seen from F up to β of Dr. Huggins' series of lines typical of the photographic spectra of the white stars. Six of these lines were fringed by a dark line on the more refrangible side. The average displacement of the dark lines as compared with the bright was 18·3 tenth-metres; that would correspond to a motion of a little over 800 miles per sec., but owing to the want of sharpness of the lines these measures will probably not be very precise. I was able to get a view of the visual part of the spectrum with the same instrument, and was able to recognize as bright lines the C line of hydrogen, the D line, two lines between C and D, and five other lines, the most refrangible of which would be F, besides some lines further in the violet, which were seen on the photograph. That is as much as I have been able to see in the spectrum of this star. Personally I felt it as exceedingly tantalizing that it should have come at a time when I was weaponless to deal with it properly.

The Astronomer Royal. I should like to ask Mr. Lockyer and Mr. Maunder one question, and that is, How the wave-lengths of these lines have been determined?

Mr. Lockyer. The centre of the bright hydrogen lines were taken to be correct, and the other wave-lengths obtained from them.

Mr. Maunder. I photographed the spectrum of β Tauri side by side with that of the Nova, and the series of bright lines of which I spoke corresponded closely to the dark lines in β Tauri. The lines in β Tauri are those of hydrogen; I therefore assumed the wave-lengths Dr. Huggins has taken for the hydrogen lines for the principal bright lines in the Nova, and computed the wave-lengths of the other lines by reference to them.

Mr. Turner then read a paper by *Prof. Pritchard*—"Observations of the Magnitude of Nova Aurigæ." Prof. Pritchard says in this paper that the Oxford determinations of magnitude depend upon observations with the wedge-photometer and on photographs taken with the 13-inch photographic telescope. χ Aurigæ, magnitude 5·08, φ Aurigæ, 5·44, 26 Aurigæ, 5·63, and L 10143, 5·84, were used as comparison stars. The wedge-photometer results show a fall from 4·82 on Feb. 3 to 5·11 on Feb. 5, and a rise to 4·96 on Feb. 7, after which the magnitude fell steadily to 5·60 on Feb. 18, rising to 5·51 on Feb. 22, since which date it has diminished steadily to 5·89 on March 7. The photographs show a fall from 5·09 on Feb. 11 to 5·70 on Feb. 18, since which date only one determination has been made—5·82 on March 7.

The Chairman. We have been observing the magnitude at Oxford, and our results agree very closely with those of Mr. Knott. According to estimates I made it was nothing like the magnitudes which have come out in the photographs at Greenwich.

The Astronomer Royal. The photographic plate is different to the eye, and photographic magnitudes cannot be expected to always agree with visual ones.

Mr. Taylor. Most of the observations which we have been able to make with the 5-foot reflector at Ealing are already familiar to most of the Fellows, having been published in 'The Observatory' for March. There is one point, however, which is of considerable importance, and that is with regard to the appearance of the bright lines with the narrowest slit possible in observing the spectrum. The observation was made at the request of Prof. Lockyer, who had found the lines broad and fluffy in his photographed spectra, and wished to know whether this was due to any nebulous fringe round the star or to internal commotions. With the narrowest slit it was possible to use we found the lines perfectly sharp and without wings or fringe of any kind. The dark lines were more difficult to observe than the bright ones, and with a fairly wide slit appeared broader than the corresponding bright ones. The positions of the lines were not measured with sufficient accuracy to justify wave-lengths being given, but the rough comparisons made fully agree with Mr. Maunder's visual observations. Thinking that the line in the green near 5005 might be the well-known nebular line, a careful search was made for the 495 line almost always associated with the 5005 in nebulæ; but no line was found in that position.

Father Sidgreaves. Mr. Taylor has raised a very important question: whether the breadth of the bright bands in the Nova is real or not. The two plates shown on the screen by Professor Lockyer go to show that the breadth of the lines in the Nova is natural, because those of Arcturus are not widened. All my own plates of the spectrum of Nova, taken at various altitudes of the star and in very different atmospheric conditions, show the same breadth of the lines; and my experience at Stonyhurst has been uniform through nearly two years, that, excepting a slight blurring of the lines at times, the same star always shows its lines in the same character independently of atmospheric conditions. The conclusion rather forces itself that the photographic plate reveals a character of the lines which the eye cannot perceive.

The Astronomer Royal gave a brief introduction to a paper "On the Large Sun-spot of Feb. 5–17 and the associated Magnetic Disturbance." The large spot-group on the Sun from Feb. 5–17 was the largest ever photographed at Greenwich, and its presence was associated with a great magnetic disturbance. Some particulars of the spot-group may be of interest, though they must necessarily be imperfect pending the arrival of photographs from India and Mauritius to supplement the Greenwich series. Mr. Maunder will give an account of the slides of the sun-spot prepared from the Greenwich photographs; and I have asked Mr. Ellis to give an account of the magnetic storm, together with a list of magnetic disturbances in recent years, a large number of which seem to have been accompanied by sun-spots of an exceptional size or by some remarkable change in spots on the Sun. One remark I would make on the connection between sun-spots and magnetic disturbances, and that is that we must bear in mind that we cannot see what is going on on the other side of the Sun, and something may be going on there which may be the real cause of magnetic disturbances not associated with any large spots on the visible face of the Sun.

Mr. Maunder explained some photographs of the great group which were then thrown on the screen. The first photograph was taken on February 5, when the group had just come into view at the east limb. The total area of the group was then about 1522 millionths of the Sun's visible hemisphere. The area of the group had nearly doubled by the time of the next photograph, which was not taken until February 13. The area on this day was just 3000 millionths. (Besides the photograph showing the group on a large scale, a second was shown giving the entire disk, so as to bring out the proportion of the group to the entire Sun.) The group showed a rapid diminution in size after this, its area being 2288, 1433, and 1389 on the 16th, 17th, and 18th respectively. (Photographs of the group on the last two days were shown, the group being close to the west limb on the latter date.) Mr. Maunder went on to say that the group had been seen again at the east limb on March 5. It had shrunk to very small proportions by this time, the total area of the group being only 208 millionths and of the principal spot 101, one thirtieth, that is to say, of what it was on February 13. The group, however, tended to revive again during the next few days, and on March 7 and 8 its area exceeded 500 millionths. The heliocentric latitude of the group had remained practically unchanged throughout, being roughly − 29°. The longitude appeared to show a slight diminution, but this is due to the rate of revolution of the Sun being less in high latitudes than the 25·38 days sidereal period used in the computations. Photographs of the spot-groups of November 1882 and of April 1882, the only groups in the Greenwich record comparable with the present one as to size, were then shown. The group of November 1882 had an area of 2425 millionths. The April photographs showed two great groups on the Sun at the same time, one with an area of 2258, the other with one of 2109. Simultaneously with these three great solar outbursts there had occurred the three greatest magnetic disturbances which had been recorded during the entire time that regular sun-spot observations had been carried on at Greenwich.

Mr. Ellis said that magnetic disturbances had much increased during the last year or two. The year 1890 was very quiet, as is always the case near to the times of minima of sun-spots; in 1891 there began to be activity, and during the late winter there has been considerable disturbance, culminating in the great disturbance of February 13. Other disturbances indeed occurred on January 5, March 1, and March 6. The disturbance of February 13 compares with those of 1882 April and 1882 November. That of 1882 November was probably the greater. The disturbance of 1892, February 13, commenced at $5^h\ 32^m$ Greenwich Civil Time, and terminated on the evening of February 14. Sometimes the magnetic outburst consists of two storms with a calm period of some hours between. This was the case in 1882 April and November, in 1892 February there was no such break. (A lantern representation of the disturbance of February 13 was here shown, and the variations, which were large in all elements, explained.) In regard to the endeavour to trace connection between large sun-spots and magnetic disturbances, it appears that one or more large disturbances will usually occur during the life of any unusual sun-spot; but we are unable to say why a disturbance should occur more at any one time than another, as they will sometimes be considerable with the spot on the other side of the Sun. A large sun-spot is probably the visible effect of some influence we cannot directly measure; so also with the magnetic disturbance. We seem to be comparing two effects, and not either one with the direct cause. A curious point is that magnetic disturbance seems to be more frequent in spring and autumn than at other times. Thus, taking for the last 50 years the Greenwich records, disturbances are two or three times as frequent before and after the vernal or autumnal equinox than at the solstices. But magnetic disturbances depend generally on the rise and fall of sun-spots, in which we do not expect to find an annual period; so that something seems to depend on the Earth's orbital position. Magnetic disturbances frequently commence with great suddenness; the greatest movements in 1882 April and 1892 February occurred at night, the greatest in 1882 November at noon. Magnetic phenomena open out many interesting questions for discussion.

Captain Steele. Does the magnetic disturbance attributed to sun-spots take place at the same moment over the entire surface of the Earth, or when the Earth's diurnal motion brings the sun-spots in line of observation?

Mr. Ellis. The disturbance takes place at the same absolute time over the entire Earth.

Mr. Inwards. I should like to learn from Mr. Ellis whether he has recorded any considerable magnetic disturbances during the time any of these large spots were moving round on that side of the Sun turned away from the Earth?

Mr. Ellis. The disturbance usually occurs when the spot-group is at or near the central meridian of the Sun's visible disc, and I do not know of any case where a magnetic storm was directly connected with a large group known to be on the side of the Sun turned away from us.

Mr. Isaac Roberts exhibited by the lantern on the screen two negatives of *Sirius* showing a number of streaks round the star. The streaks resembled those which he found amongst the stars in the group of the Pleiades, but he did not think they indicated nebulosity round *Sirius*, and he had not made up his mind to what cause they were to be attributed. He is engaged investigating the subject, and at present would not pronounce his opinion, but the streaks were both remarkable and suggestive.

The Astronomer Royal. The question of arranging some local centre, in communication with the "Centralstelle" at Kiel, for distributing information about astronomical discoveries has been mentioned to-night. I do not think it would be difficult to arrange for a number of English observatories to send a joint subscription to the "Centralstelle" at Kiel, and then to distribute the information received from that organization amongst themselves from a local centre, thus saving the expense of separate telegrams from Kiel. The only requirement is a centre where some one will be always on the spot to receive and distribute news, and if a suitable place of this kind can be suggested there ought to be no difficulty of any kind. This arrangement has, I believe, been carried out in Russia, where Pulkowa distributes to the other Russian observatories; but this is a work which could not advantageously be undertaken in this country at Greenwich, as there would be considerable loss of time in transmission from London to the Observatory at Greenwich and back, and there would be delay at times when there happened to be no one on the spot available for the purpose.

The following papers were announced:—

G. Comstock. "The Reappearance of Saturn's Ring."

J. Kleiber. "Displacement of Apparent Radiant Points of Meteor Showers, due to the Attraction, Rotation, and Orbital Motion of the Earth."

S. W. Burnham. "Observations of Nebulæ with the 36-inch Refractor of the Lick Observatory."

S. W. Burnham. "A new Binary Star, β 581."

J. Tebbutt. "Results of Double-Star Measures at Windsor, New South Wales, in 1891."

J. Mitchell. "Maxima and Minima of Variable Stars observed during the years 1889–91."

Rev. A. Freeman. "Occultation of γ_1 and γ_2 Virginis."

A. M. W. Downing. "The apparent places of close Polar Stars."

Isaac Roberts. "Photograph of the Region of Nova Cygni."

Isaac Roberts. "Photographs of the Region of Nova Aurigæ."

W. H. M. Christie. "Photographic Magnitude of Nova Aurigæ as determined at the Royal Observatory, Greenwich."

"On the Large Sun-spot of Feb. 5–17, and the associated Magnetic Disturbance." Communicated by *The Astronomer Royal.*

George Knott. "The New Star in Auriga."

Prof. C. Pritchard. "Preliminary Note on the Magnitude of the New Star in Auriga."

A. Marth. "Ephemeris for Physical Observations of Mars, 1892."

A. Marth. "Ephemeris of the Satellites of Saturn, 1891–92."

E. W. Maunder. "Note on the Spectrum of Nova Aurigæ."

H. H. Turner. "On an Annual Inequality in the R–D Correction."

Prof. G. W. Hough. "Observations of the Spots and Markings on the planet Jupiter made at the Dearborn Observatory."

Arthur Gibbons, Science, Art, and Technical School, Brierly Hill, Dudley, was duly elected a Fellow of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

Rev. W. H. Addison, 8 West Chapel Street, Mayfair, W. (proposed by E. J. Spitta); *Henry Baynham*, Lieut. R.N., Captain of

H.M.S. 'Wellesley' Training-ship (proposed by David Gill); *Humphrey Barker Chamberlin*, 1033 Sixteenth Street, Denver, Colorado, U.S.A. (proposed by H. H. Turner); *Otto Jaffe*, German Consul, Kin Edar, Strandtown, Belfast (proposed by J. L. E. Dreyer); *Charles Henry Johns*, M.A., Althorpe House, Waverley Grove, Hendon, Middlesex (proposed by J. D. McClure); *William Grant MacGregor*, 18 Coleman Street, E.C. (proposed by Robert Grant); *Capt. R. Reynolds*, Lieut. R.N.R., U.S.S. 'Pretoria,' Southampton (proposed by D. Forbes); *Albert Edward Watson*, B.A., F.R.Met.Soc., Whitgift Grammar School, Croydon (proposed by Charles B. Neate).

BRITISH ASTRONOMICAL ASSOCIATION.

THE fourth ordinary Meeting of the second session was held on Wednesday, Feb. 24, at Barnard's Inn Hall, Holborn, Capt. Noble, the President, in the Chair. Fourteen new Members were elected, and the names of nine Candidates for election were read and suspended.

The Rev. T. E. Espin gave an account of his researches on the New Star in Auriga. He stated that the most remarkable lines were: an intense black one on the more refrangible side of D; a great number of bright lines in the yellow, a bright line which he thought was the chief nebular line; a similar line which Prof. Lockyer considered to be the second nebular line, but Mr. Espin thought the second nebular line should be nearer the red. Next came four bright lines close together, having their edges towards the violet perfectly sharp and towards the red less distinct, which on further magnification seemed to be rather flutings than lines. After Feb. 16 the D line had gradually faded. On Feb. 2 he made its magnitude 5·6, and it gradually rose till on the 8th it was 4·7. After this it again diminished till on the 20th it was 5·8.

The President remarked that he had observed the star a few days and noticed its change of brightness. Mr. Ranyard asked whether Mr. Espin could give any evidence supporting the theory of its being a binary star.

Mr. Maunder said that from a photograph of the spectrum taken at Greenwich, he thought the bright lines were deflected towards the red as compared with the dark lines; this was borne out by a photograph taken at S. Kensington, and this would indicate that the star was a binary, whose components had a relative velocity in the line of sight of 500–600 miles per second.

Mr. Cottam read a paper by Miss E. Brown on the Great Sunspot Group of Feb. 5–18, illustrated by lantern slides taken from her drawings.

Mr. Holmes and Mr. Adams called attention to the fact that observers did not take any note of the depth of the nuclei of Sun-spots, which would be necessary in order to obtain definite results with regard to their magnetic effects.

In reply to a question as to whether magnetic effects had been noticed, Mr. Adams stated that the telegraph lines were greatly affected on the afternoon of the 13th, when the Sun-spot was on the meridian.

Mr. Maunder stated that a sharp disturbance of the magnetic instruments at Greenwich had taken place on Feb. 13, and he also said that the group was the largest ever photographed at Greenwich.

Miss Clerke pointed out that great Auroræ were seen in America.

Mr. Cottam stated that on Saturday evening, Feb. 13, the whole western sky appeared of a bright rose-colour.

ROYAL METEOROLOGICAL SOCIETY.

At the Meeting of this Society on Wednesday, March 16, Dr. C. Theodore Williams, the President, delivered an Address on the Value of Meteorological Instruments in the Selection of Health Resorts. He drew attention to Thermometers, maximum and minimum, as the foundation-stone on which Medical Climatology rests, and instanced effects of extreme cold or heat on the human organism. The direct rays of the Sun are of the greatest importance, and in health resorts should be utilized to the full; in fact, only climates where during the winter months even a delicate person can lie or sit for several hours a day basking in the sunshine are to be recommended for most complaints, and the various forms of sunshine-recorders are used to aid the medical adviser in choice of such health-stations. After referring to the value of rain-gauges, hygrometers, and barometers, Dr. Williams stated that many health-resorts owe their reputation almost solely to their shelter from cold winds; for instance, the advantage in climate which Hyères and Mentone enjoy over Marseilles is chiefly due to their being more sheltered from the mistral or north-west wind, the scourge of the lower valley of the Rhone from Valence to Avignon. He went on to describe the climate of the Riviera, illustrating it by lantern-slides from recent photographs, including views of Hyères, Costabella, Cannes, Nice, Mentone, San Remo, &c.; and he showed the three principal causes of the warm winter of this region to be:—(1) the southern latitude; (2) the protection from cold winds by mountain-ranges; and (3) the equalizing and warming influence of the Mediterranean Sea, which being practically tideless is always equally potent, not varying with hour and season. Dr. Williams mentioned the weak points of the South of France climate with its blustering mistral, its occasional *bise*, its moist sirocco wind, but summed up the Riviera winter climate as being, as a whole, clear, bright, and dry, with fog and mist practically unknown, with a winter temperature of 8° to 10° higher than England, though subject to considerable nocturnal radiation, with about half the number of rainy days and four or five times the number of bright ones which we can boast of, with cold winds and cold weather, without which it would lose its healthgiving effects.

After the delivery of this address the Meeting was adjourned in order to allow the Fellows and their friends an opportunity to inspect the Exhibition of Instruments relating to Climatology, which had been arranged in the rooms of the Institution of Civil Engineers, 25 Great George Street. The Meteorological Office showed a set of instruments necessary for the equipment of a climatological station, viz. Stevenson thermometer-screen, fitted with dry-bulb, wet-bulb, maximum and minimum thermometers, and also a rain-gauge. Thermometers were also shown for ascertaining the temperature on the ground, under the ground, and at a distance, as well as for recording temperature continuously. Various forms of sunshine-recorders were exhibited, as well as a number of actinometers and solar radiation instruments for ascertaining the heating effect of the solar rays. The Exhibition included a large and interesting collection of hygrometers. also several rain-gauges and other instruments. Among the curiosities was a piece of plate-glass which was "starred" during a thunderstorm on August 21, 1879; this was not broken, but it has a number of wavy, hour-like lines. The Exhibition contained a large number of beautiful photographs of cloud, lightning, and snow-scenes, as well of the damage done by the destructive tornado at Lawrence, Mass., U.S.A. The Exhibition remained open until Tuesday, the 22nd.

Selenographical Notes.

Posidonius.—Apart from the beautiful telescopic picture which this large ring-plain presents during the early stages of sunrise, it will repay careful examination on account of the manifold details it includes, and the curious group of craters with which it is associated on the north. There are, besides, records, from the time of Schröter downwards, of anomalous appearances in connection with objects in the interior, which seem to justify a suspicion that something of the nature of an obscuring medium has from time to time affected their visibility, and has otherwise modified their usual aspect, which render it very desirable that it should be kept under close observation.

Perhaps the most pleasing and satisfactory view of the formation and its surroundings is obtained when the west long. of the morning terminator is about 24°. The floor at this phase being practically free from obscuring shadows,—the deep central crater

A with its neighbouring hills, the remarkable ridges and clefts in the interior, and the black projections of the peaks on the eastern wall on the grey surface of the Mare Serenitatis are then observed to the best advantage.

Posidonius, like Taruntius, displays very clear indications of an inner concentric ring, which is well-marked on the west and south, and still better, for a short distance, on the south-east. The latter section ultimately runs down to the level of the floor, its direction being prolonged by a rill ξ (in Neison's Map III.) nearly up to the north-eastern border. Schmidt and Neison both show very prominently the rill η, which traverses the interior in an axial direction from north to south, and runs amid the crowd of little hills west of A. It is not, however, an easy object at any time, certainly not under morning illumination. It is generally seen best under a setting sun, about the time when the evening terminator lies near Cauchy. On the north it abuts at right angles on what appears to me to be a crater-rill, though Schmidt in the place of this object draws a curved mountain. Neison's cleft, ψ, answers to the western section of it; but at 7^h on February 3, this year, it was traced to a point more than halfway between the south end of η and the gap κ in eastern wall. The cleft at the southern end of η is always a more or less conspicuous feature under oblique light. What is commonly seen, is, I think, a coarse valley between parallel ridges and not the delicate crater row shown by Schmidt north of these objects.

Schröter devoted much attention to Posidonius and the vicinity, and during the course of his observations noticed on several occasions a very remarkable phenomenon in connection with the deep crater A on the floor, viz., at a time when, according to the phase that prevailed, it should have been filled with black shadow, no trace of any shadow was apparent. Many years after this, on April 7, 1821, when the formation was under a low rising sun, Gruithuisen saw a similar appearance. Though the details on the floor were distinctly defined, the great central crater contained no shadow in its interior and so few indications of any shade, that its place could only be detected with difficulty. Yet, strange to say, on the next night the crater A included a distinct shadow and presented its usual aspect. Mädler is said to have often looked for this remarkable appearance, but never succeeded in seeing it. Whatever views may be held respecting the existence of an appreciable lunar atmosphere, the probability of the emission of gases from a body like the Moon, which almost everywhere displays such unmistakable signs of the results of the reaction of the interior on the exterior surface, will hardly be questioned. Those who have given much attention to the observation of clefts and other small details frequently meet with anomalies relating to the visibility of these objects which can apparently only be accounted for by supposing that a vaporous veil of some kind (due to no terrestrial cause) enshrouds and conceals them from sight.

Many of Gruithuisen's observations, especially those relating to

John Couch Adams

from a photograph by Mrs F. W. H Myers

clefts, were for a long time regarded as the results of his lively imagination, and were consequently received with scepticism; but the lunar work of the last 25 or 30 years in this direction has very generally confirmed the accuracy of his records and has certainly tended to add to his reputation as an observer. Both he and Schröter were perhaps over credulous on the subject of visible changes on the lunar surface, while Mädler was unduly biassed on the opposite side. Dogmatism in the present imperfect state of selenography ought to have no place in a question which should be approached with an open mind. Gruithuisen was so excellent and experienced an observer that his circumstantial confirmation of Schröter's statements relating to Posidonius A, instead of being passed over as worthless, should lead to a more sustained and critical scrutiny of lunar detail, in this and other quarters, than has hitherto been made.

The large ring-plain Daniell with its central mountain, the group of craters in the region immediately north of Posidonius, and the depressions closely associated with the wall, are all noteworthy features,—the crater I, and a second, somewhat smaller, to the north of it causing an interruption in its continuity. Lohrmann discovered a minute crater on the northern rim of the former, which was not again observed till it was re-detected in 1876, by Mr. Sadler.

A small crater south of Posidonius *b* appears to form the northern termination of the great serpentine ridge which extends from this point to the Prom. Acherusia near Pliny. The curious details of the northern section of it may be well studied when the western long. of the morning terminator is about 25°. The structural connection between Posidonius and Charconac is not satisfactorily represented in any of the maps, nor are the very difficult details within the latter formation correctly shown.

Beaumont House, Shakespeare Road,
Bedford, 1892, Mar. 19.

T. Gwyn Elger.

JOHN COUCH ADAMS.

By the death of Professor Adams, England has lost the greatest mathematical astronomer she has ever produced, Newton alone excepted. The analytical mechanism required to trace the consequences of the Newtonian law has been devised and developed almost exclusively upon the Continent, and Newton's own countrymen have had but little share in carrying on his work. The names of Clairaut, Euler, Lagrange, Laplace, and Gauss follow in natural sequence upon that of their great master; but till we come to Adams, there is no Englishman who can claim a place in this illustrious succession. His discovery of the planet Neptune by mathematical calculation, his memoir upon the secular acceleration of the Moon's mean motion, and his determination of the orbit of the November meteors, already take their place in the history of astronomy. He was a consummate master of all the refined and delicate methods with which the genius of the greatest mathematicians has endowed the subject of celestial mechanics, as well as of the detailed processes by which theory and observation are connected. No mathematician of our time has shown greater power or versatility in the treatment of the many difficult problems presented to us by the lunar and planetary theories, and it may safely be said that no mathematical astronomer was ever more sure of arriving at accurate conclusions by methods of such complication and difficulty.

John Couch Adams was born on June 5, 1819, at the farmhouse of Lidcot, seven miles from Launceston, in Cornwall. His father was a tenant farmer, and his mother possessed a small estate of land of her own. She had also inherited her uncle's library, and these books, which included some on Astronomy, were his early companions. At the village school at Laneast he made rapid progress, and was learning Algebra before he was twelve years old. At this age he went to a private school at Devonport, kept by the Rev. John Couch Grylls, a first cousin of his mother's.

He remained under Mr. Grylls's tuition for a good many years, first at Devonport and afterwards at Saltash and Landulph, and received the usual school training in Classics and Mathematics. Astronomy has been his passion from very early boyhood, and at 14 years of age he made copious notes and drew tiny maps of the constellations. He read with avidity all the astronomical books to which he could obtain access, and in particular he studied the astronomical articles in Rees's Cyclopædia, which he met with in the library of the Devonport Mechanics' Institute. In the same library he came across a copy of Vince's Fluxions, which was his first introduction to the higher Mathematics.

He showed such signs of mathematical power, that in 1837 the idea of his going to Cambridge was entertained. He accordingly entered St. John's College, Cambridge, in October 1839. During his undergraduate career he was invariably the first man of his year in the College examinations, and in 1843 he graduated as Senior Wrangler, being also first Smith's Prizeman. In the same year he was elected a Fellow of his College.

His attention was drawn to the irregularities in the motion of Uranus by reading Airy's Report upon recent progress in Astronomy in the British Association volume for 1832–33, and on July 3, 1841, he made the following memorandum:—"Formed a design at the beginning of this week of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus which are yet unaccounted for; in order to find whether they may be attributed to the action of an undiscovered planet beyond it; and if possible, thence to determine the elements of its orbit &c. approximately, which would probably lead to its discovery." This memorandum was made at the beginning of his second long vacation, when he was in his twenty-third year.

In 1843, the year in which he took his degree, he attempted a first rough solution of the problem on the assumption that the orbit was a circle with a radius equal to twice the mean distance of Uranus from the Sun. The result showed that a good general agreement between theory and observation might be obtained. In order to make the data employed more complete application was made through Professor Challis to the Astronomer Royal for the results of the Greenwich observations of Uranus. When these were obtained, Adams undertook a new solution of the problem, taking into account the most important terms depending on the first power of the eccentricity of the orbit of the supposed disturbing planet, but retaining the same assumption as before with respect to the mean distance. In September, 1845, he communicated to Professor Challis the values which he had obtained for the mass, heliocentric longitude, and elements of the orbit of the assumed planet. The same results, slightly corrected, he took with him to the Royal Observatory, Greenwich, on October 21, 1845. The paper which he left at the Observatory on this occasion also contained a list of the residual errors of the mean longitude of Uranus after taking account of the disturbing effect of the new planet, at dates extending from 1690 to 1840.

On November 10, 1845, Leverrier presented to the French Academy an elaborate investigation of the perturbations of Uranus produced by Jupiter and Saturn, in which he pointed out several small inequalities which had previously been neglected. After taking these into account, and correcting the elements of the orbit, he still found that the theory was quite incapable of explaining the observed irregularities in the motion of Uranus.

On June 1, 1846, Leverrier presented to the French Academy his second memoir on the theory of Uranus, in which he concluded that the unexplained irregularities in the motion of Uranus were due to the action of an undiscovered planet exterior to Uranus. He investigated the elements of the orbit of such a planet and, assuming its mean distance to be double that of Uranus, and its orbit to be in the plane of the ecliptic, he found that the most probable value of the true longitude of the disturbing body for the beginning of 1847 was about 325°, but he did not give the elements of the orbit or the mass of the planet.

The place thus assigned by Leverrier to the disturbing planet differed by only 1° from that given by Adams in the paper which he had left at the Greenwich Observatory seven months earlier.

Professor Challis commenced the search for the planet with the Northumberland telescope of the Cambridge Observatory on July 29, 1846, three weeks before the planet was in opposition, and the observations were continued for two months. His plan was to examine a zodiacal zone having its centre in the ecliptic at 325° of longitude, and extending 15° of longitude in each direction from the central point, and from 5° north latitude to 5° south latitude. He proposed to make two sweeps over each portion of the zone, so that, when the observations were compared, a planet could be at once detected by its motion in the interval. For the first few nights the telescope was directed to the part of the zone in the immediate neighbourhood of the place indicated by theory. Unfortunately the observations were not immediately compared with each other, or Professor Challis would have discovered, what he found afterwards to be the case, that he had actually observed the planet on August 4 and August 12, the third and fourth nights of observation. The star map of the Berlin Academy for Hora xxi. of right ascension had lately been published, but the English astronomers were not aware of its existence. By the help of this map the search would have been extremely easy and rapid, as the observations could have been compared with the map as fast as they were made.

On September 3, 1846, Adams communicated to the Astronomer Royal a new solution of the problem, supposing the mean distance of the planet as originally assumed to be diminished by about the $\frac{1}{30}$th part. The result of this change was to produce a better agreement between the theory and the later observations, and to give a smaller and therefore a more probable value of the eccentricity. Meanwhile, on August 3, 1846, Leverrier had presented to the French Academy his second paper upon the place of the disturbing planet, which, however, did not reach this country till the third or fourth week in September. In this elaborate paper Leverrier obtained elements of the orbit of the disturbing planet which are very similar to those obtained in Adams's second solution. Leverrier communicated his principal conclusions to Dr. Galle, of the Berlin Observatory, in a letter received by him on September 23, 1884; and, by comparing his observations with the Berlin star-map, Dr. Galle found the planet on the same evening.

Adams's researches therefore preceded Leverrier's by a considerable interval; and, in spite of the delay in commencing the search, it had been carried on at Cambridge for nearly two months before the planet was found at Berlin. Adams's investigation may be regarded as having been completed on October 21, 1845, when he left his paper at the Royal Observatory. This was three weeks before Leverrier's memoir, showing that the irregularities could not be attributed to the known planets, was presented to the French Academy, and more than seven months before the date of presentation of his second memoir. It is to be noticed also that in this second memoir Leverrier did not give the elements of the orbit or the mass of the planet which were contained in Adams's paper of October 21. As we know, Adams had resolved to undertake the work in 1841, and his first solution was effected, as soon as he had leisure, in 1843. It is to be supposed that Leverrier did not attempt the actual solution until after the completion of his memoir of June 1, 1846.

Although the conception of universal gravitation was present to Newton's mind in 1665, we may date the beginning of the investigations which ended in the publication of the 'Principia' from the letter which Newton wrote to Hooke on Nov. 28, 1679, by which he was led to consider the form of an orbit. In consequence of Halley's visit to him at Cambridge in 1684, he returned to the subject and began the 'Principia,' which was completed and published by July 5, 1687. Then followed the years when Newton's law was on its trial, and we may fairly say that the final and inexorable proof had been attained when, on October 21, 1845, Adams took to Greenwich the paper by means of which the new planet might have been at once discovered. This date may therefore be regarded as marking a distinct epoch in the history of gravitational astronomy *.

There is something romantic in the fact that this brilliant result was effected by so young a man. The problem was one of great difficulty †; he had no help; and there were no books. It is interesting to notice the confidence he felt in the sufficiency of the Newtonian law to account for all the irregularities in the motion of Uranus; absolutely rejecting every other hypothesis, and relying implicitly upon this law alone, he undertook his laborious and difficult investigation and carried it through with complete success. He was convinced of the reality of the planet and of the approximate accuracy of the place he had assigned to it; and in the paper which he placed in the hands of Professor Challis in September 1845, he actually used the words "the new planet." We cannot fail to admire the extreme modesty which he displayed in connection with so great an achievement--a modesty which was characteristic of him throughout his whole life. He has been blamed for not publishing his results, as he was advised to do by his Cambridge friends, and so securing his own share in the discovery; but he had no thought of reaping distinction for himself. He believed that by placing his results in the hands of the Astronomer Royal he was taking a course which would lead to a search being made, at Greenwich or elsewhere, which would shortly lead to the actual detection of the planet.

Of the bitter and painful controversy that arose in connection with respect to the discovery of Neptune it is unnecessary to speak here ‡. It is sufficient to say that Adams took no part whatever in the discussion either then or at any subsequent time; and his own silence, and his known dislike to be the subject of any controversy, restrained his friends all through his lifetime from giving expression to the opinions they may have held. He never wrote or said an unkind word with reference to anyone concerned in the controversy. Of Leverrier's work he

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* In Professor Challis's words, "The earliest evidence of the complete solution of an inverse problem of perturbations is to be dated from October 1845."

† Writing to Bouvard, Oct. 12, 1837, Airy said:—"If it [the error of longitude in Uranus] be the effect of an unseen body, it will be nearly impossible ever to find out its place."

‡ On this subject the reader may consult Airy's article in vol. xvi. of the Mem. R. A. S. and an article by Sir David Brewster in vol. vii. (1847) of the 'North British Review.'

expressed from the first the warmest appreciation *, and it was a great pleasure to him when they met at Oxford in 1847. In the same year Leverrier visited him at Cambridge.

At the Meeting of the Royal Astronomical Society on November 13, 1846, Adams communicated a memoir containing a full account of the mathematical investigations in connection with the determination of the mass, orbit, and position of the new planet, by which he had obtained the results communicated to the Astronomer Royal on October 21, 1845, and September 3, 1846. As it was thought desirable that this paper should be published without delay, it was printed at once as an appendix to the 'Nautical Almanac' for 1851, in anticipation of the publication of vol. xvi. of the 'Memoirs' of the Society, in which it appeared a few months later in 1847 ‡. At the same Meeting also Professor Challis gave an account of his observations undertaken in search of the planet.

Soon after the discovery of Neptune several members of St. John's College raised a fund, producing about £80 per annum, for establishing a prize to be connected with the name of Adams, in testimony of their sense of the honour he had conferred on his college and the University by having been the first to determine from perturbations the unknown place of a disturbing planet. This fund was offered to the University, and accepted by Grace of the Senate, April 7, 1848. The prize is a biennial one, and since its foundation it has been awarded among others to Clerk Maxwell, Todhunter, Routh, and J. J. Thomson.

On the occasion of the Queen's visit to Cambridge, at the installation of the Prince Consort as Chancellor in 1847, the honour of knighthood was offered to Adams, but declined by him.

In the year 1848 he was occupied with the determination of the constants in Gauss's theory of terrestrial magnetism. This work he afterwards resumed, and the calculations connected with it, with which he was occupied in the later years of his life, were left unfinished at the time of his death.

In 1851 he was elected President of the Royal Astronomical Society, and held the office for the usual term of two years. In 1852 he communicated to the Society new tables of the Moon's

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* At the commemorative meeting at St. John's College on Feb. 20, 1892, Dr. Donald MacAlister said :—"Of his perfect freedom from thoughts of self, and the utter absence of any memory of bitterness in connection with that great controversy, I may be allowed to relate a single illustration, which has not hitherto been made public. Some years ago it was desired to get up in this country a memorial volume to be presented to M. Pasteur, which should indicate the appreciation felt by English men of science for his great scientific merits and for the services he had rendered to humanity. With a number of distinguished members of this University Professor Adams subscribed his name, and the motto which he chose to write beneath it was simply this: 'Hommage au compatriote de Leverrier.' The one fact that he remembered in doing honour to M. Pasteur was that he was a compatriot of a great astronomer, but one whom a meaner man would have considered his greatest rival."

‡ A French translation of this memoir was published in 1876 in Liouville's Journal, with a short appendix by Adams.

parallax to be substituted for those of Burckhardt. He showed that several of the periodical terms of the parallax as found from Burckhardt's tables were seriously in error. The new tables were printed in the appendix to the 'Nautical Almanac' for 1856.

As Adams had not taken holy orders his fellowship at St. John's College expired in 1852, but he continued to reside in the college until February 1853, when he was elected Fellow of Pembroke College. He remained a Fellow of this college up to his death.

In 1853 his now celebrated memoir on the secular variation of the Moon's mean motion was communicated to the Royal Society. It is well known that the secular acceleration which, as determined from ancient eclipses, amounts to about 10″ in a century, for a long time defied any explanation by means of the principles of gravitation. The French Academy offered their prize for such an explanation in 1770, 1772, 1774, and on each occasion it was awarded to Euler or Lagrange or both. But neither of these great mathematicians succeeded in discovering any secular term; and indeed Euler, regarding it as established that such a term could not be produced by the principles of gravitation, had recourse to the supposition of a resisting medium. In 1787, however, Laplace discovered the true cause of the phenomenon in the gradual diminution of the mean action of the Sun, produced by the secular variation of the eccentricity of the Earth's orbit. The value as calculated by Laplace agreed very closely with the amount derived from the ancient eclipses. Laplace confined his approximation to the value of the acceleration to the first term, involving m^2, but in the theories of Damoiseau and Plana the developments were carried to an immense extent, especially by the latter. As these theories agreed in principle and only differed slightly in the numerical values which they assigned to the acceleration, and as they passed under the examination of Laplace, it might be supposed that at most only some small numerical corrections would be required in order to obtain a very exact determination of the amount of the acceleration. It was found, however, by Adams that Laplace's explanation of the phenomenon was essentially incomplete, and that in consequence the numerical results in Damoiseau's and Plana's theories with reference to it required to be very sensibly altered: in fact, in the expression of the true longitude in terms of the mean, the term of the fourth order given by Plana as $\frac{2187}{128} m^4$ should be $\frac{3771}{64} m^4$.

For some time very little attention seems to have been paid to the results contained in this paper, but it then became the subject of attack by Plana, Hansen, and Pontécoulant. In 1856 Plana admitted that his theory was wrong upon the point, and actually deduced Adams's result from his own equations. Soon afterwards, however, he retracted his admission of the correctness of Adams's result, and obtained a third result differing both from his former one and from Adams's. Subsequently he discussed the matter with Lubbock, and finally recognized that Adams was right. Hansen's value of the secular acceleration was not given in an analytical form, so that only the final numerical results admitted of comparison. Pontécoulant's attacks, which extended over several years and were characterized by great virulence, appeared in the 'Monthly Notices' and the 'Comptes Rendus.' Adams was supported by Delaunay, who was led to calculate the secular acceleration by his own method, which differed completely from all of those which had been previously employed. He found the coefficient of m^4 to be exactly as stated by Adams. Not content with this verification, he made a new calculation, following the method indicated by Poisson in his memoir of 1833, and obtained the same value. Independent calculations were also made by Cayley and by Lubbock, the results in both cases confirming Adams's result. In the 'Monthly Notices' for 1860 Adams replied to the various objections that had been brought against his theory. In this reply he mentions that before publishing his memoir in 1853 he had obtained his result by two different methods, and that he had since confirmed and extended it by a third. The later attacks of Pontécoulant in the 'Comptes Rendus' for 1862, which display no abatement of violence with respect to Adams's method and results, were very effectively answered by Delaunay.

In the memoir of 1853 Adams only carried his approximation as far as the term in m^4. He subsequently calculated the coefficients of the terms in m^5, m^6, m^7. The numerical values of the successive terms he found to be

m^2	10″·66
m^4	−2 ·34
m^5	−1 ·58
m^6	−0 ·71
m^7	−0 ·25

The sum of these is 5″·78. The convergence, although slow at starting, becomes more rapid in the later terms; and Adams inferred, in a communication which he made to the French Academy in January 1859, that the remainder of the series would be very nearly equal to −0″·08, thus giving 5″·70 as the resulting value. In order to remove any possible objection which might be raised with respect to the convergency of the series, he also calculated the value of this coefficient by a method which did not require any expansion in powers of m and obtained the same value 5″·70. Delaunay calculated the next term of the series and found it to be −0″·06, which was in exact accordance with Adams's anticipations.

In Adams's calculation the terms depending upon the eccentricity and inclination of the Moon's orbit were not included. These were taken into account by Delaunay, thereby increasing the coefficient to 6″·11.

It may be added that, according to Plana's theory, the coefficient was 10″·60 and according to Hansen's 12″·76.

Twenty years afterwards, in 1880, Airy published in the

'Monthly Notices' a paper in which he obtained Laplace's original value. The error in Airy's method was pointed out by Adams, who observed that it was such as practically to reduce the series to its first term, thus accounting for the coincidence with Laplace. In the 'Monthly Notices' for June 1880 Adams gave another investigation of the secular acceleration, taking into account the terms in m^4.

On the whole, the result of the correct theory of the acceleration which we owe to Adams was to reduce its amount from about 10″ in a century to about 6″.

In the autumn of 1858 he was appointed Professor of Mathematics in the University of St. Andrews, and shortly afterwards, in the same year, he was elected Lowndean Professor of Astronomy and Geometry at Cambridge in succession to Peacock. He continued his lectures at St. Andrews, however, until the end of the session in May 1859.

In the 'Monthly Notices' for April 1867 Adams published an account of the results he had obtained with respect to the orbit of the November meteors. Prof. H. A. Newton had collected and discussed the original accounts of thirteen displays of these star-showers ranging from A.D. 902 to 1833. From these data Prof. Newton inferred that they recur in cycles of 33·25 years, and that during a period of two or three years at the end of each cycle a meteoric shower may be expected. He concluded that the most natural explanation of these phenomena is, that the November meteors belong to a system of small bodies describing an elliptic orbit about the Sun, and extending in the form of a stream along an arc of that orbit, which is of such a length that the whole stream occupies about one-tenth or one-fifteenth of the periodic time in passing any particular point. He showed that in one year this group must describe either $2 \pm \frac{1}{33\cdot25}$, or $1 \pm \frac{1}{33\cdot25}$, or $\frac{1}{33\cdot25}$ revolutions, or, in other words, that the periodic time must be either 180·0 days, 185·4 days, 354·6 days, 376·6 days, or 33·25 years. He pointed out also that the node of the orbit must have a mean motion of 52″·4 with respect to the fixed stars. Soon after the remarkable display of the November meteors in 1866 Adams undertook the examination of this question. From the position of the radiant-point observed by himself he calculated the elements of the orbit of the meteors, starting with the supposition that the periodic time was 354·6 days, the value which Prof. Newton considered to be the most probable one. The orbit which corresponds to this period is very nearly circular, and he found that the action of Venus would produce an annual increase of about 5″ in the longitude of the node, that of Jupiter about 6″, and that of the Earth about 10″. Thus the three planets, which alone would sensibly affect the motion of the node, would produce about 21″ annually, or nearly 12′ in 33·25 years. The observed motion of the node is 52″·4 annually, or about 29′ in 33·25 years, which is therefore quite inconsistent with the supposition that the periodic time of the meteors about the Sun is 354·6 days. If the periodic time were supposed to be about 377 days, the calculated motion of the node would differ very little from that in the case already considered, while if the periodic time were a little greater or a little less than a year, the calculated motion of the node would be still smaller. Hence, of the five possible periods indicated by Prof. Newton four were incompatible with the observed motion of the node, and it only remained to examine whether the fifth period of 33·25 years would give a motion in accordance with observation. In order to determine the secular motion of the node in this orbit the method given by Gauss, in his memoir 'Determinatio Attractionis, &c.' was employed. In this memoir it was shown by Gauss that if two planets revolve about the Sun in periodic times which are incommensurable with each other, the secular variations which either of these bodies produces on the element of the orbit of the other would be the same as if the whole mass of the disturbing body had been distributed over its orbit in such a manner that the portion of the mass distributed over any given arc should be always proportional to the time which the body takes to describe that arc. When the attraction of this elliptic ring has been calculated for any point in the orbit of the meteors, it is possible readily to deduce the changes which it would produce in the elements of the orbit, while the meteors are describing any given small arc contiguous to the given point. Thus, by dividing the orbit of the meteors into a a number of small portions, and summing up the changes corresponding to these portions, the total secular changes of the elements produced in a complete period of the meteors may be determined. By proceeding in this manner Adams found that during a period of 33·25 years, the longitude of the node is increased by 20′ by the action of Jupiter, nearly 7′ by the action of Saturn, and about 1′ by that of Uranus. The other planets were found to produce scarcely any sensible effects, so that the entire calculated increase of the longitude of the node is about 28′, according very closely with the observed amount of 29′ and leaving no doubt as to the correctness of the period of 33·25 years. In order to obtain a sufficient degree of approximation it was requisite to break up the orbit of the meteors into a considerable number of portions, for each of which the attractions of the elliptic rings corresponding to the several disturbing planets had to be determined. These calculations were therefore of necessity very long, although a modification of Gauss's formula was devised which greatly facilitated its application to the actual problem. Subsequently Adams subdivided certain parts of the orbit of the meteors into still smaller portions, with the view of obtaining a closer approximation. Unfortunately the mathematical investigations which he carried out on this subject were not published. They exist among his papers, together with a very great amount of numerical work connected with the calculations, and seem to be in a fairly complete state.

In 1877 Mr. G. W. Hill, of Washington, published a memoir on the part of the motion of the Moon's perigee which is a function of the mean motions of the Sun and Moon. Mr. Hill's object was to determine with absolute accuracy the motion of the Moon's perigee on the assumption that the Sun's orbit has no eccentricity, that the Moon's motion takes place in the plane of the ecliptic, and that the eccentricity of the Moon's orbit is indefinitely small; in other words, to determine an absolutely accurate value of that part of c which depends upon m alone. The development in powers of m, besides being extremely difficult and laborious to obtain, was from its nature ill adapted for the calculation of c with all the precision that could be desired. Mr. Hill accordingly had recourse to an entirely different method of obtaining the numerical value of this quantity, in which he was entirely successful, obtaining the value, by the development of an infinite determinant, to no less than fifteen places of decimals*.

The publication of Mr. Hill's memoir led Adams to make a communication to the Astronomical Society in November 1877, in which, after bearing testimony in the warmest terms to the high merits of Mr. Hill's investigation, he mentions that he had himself calculated c to the same order of magnitude, obtaining a result agreeing with Mr. Hill's. He gives in the same paper the value of g which he had calculated by a similar method also to fifteen places of decimals.

This paper is peculiarly interesting, as in it he expresses his own views with respect to the mathematical treatment of the problem of the Moon's motion. He states that he has long been convinced that the most advantageous way of treating the lunar theory is, first, to determine with all desirable accuracy the inequalities which are independent of e, e', and γ, and then in succession to find the inequalities which are of one dimension, two dimensions, and so on with respect to these quantities. Thus, the coefficient of any inequality in the Moon's coordinates would be represented by a series arranged in powers and products of e, e', and γ; and each term in this series would involve a numerical coefficient which is a function of m alone, and which admits of calculation for any given value of m without the necessity of developing it in powers of m. This method is particularly advantageous when we wish to compare our results with those of an analytical lunar theory such as Delaunay's, in which the eccentricities and the inclination are left indeterminate, since each numerical coefficient could be compared separately with its analytical development in powers of m. As it is only the series proceeding by powers of m in Delaunay's theory which have a slow rate of convergence, it is probable that all the sensible corrections required by Delaunay's coefficients would be found among the terms of low order in e, e', and γ.

He proceeds: "The differential equations which would require solution in these successive operations after the determination of the inequalities independent of the eccentricities and inclination would be all linear and of the same form.

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* The value derived from Delaunay's expression in powers of m, which extends as far as m^9, is only correct to four places of decimals. It would probably be necessary to extend the series as far as m^{27} in order to obtain a result as accurate as Mr. Hill's.

"It is many years since I obtained the value of these last-named inequalities to a great degree of approximation, the coefficients of the longitude expressed in circular measure, and those of the reciprocal of the radius vector, or of the logarithm of the radius vector, being found to ten or eleven places of decimals.

"In the next place, I proceeded to consider the inequalities of latitude, or rather the disturbed value of the Moon's co-ordinate perpendicular to the ecliptic, omitting the eccentricities as before, and taking account only of the first power of γ.

"In this case the differential equation for finding z presents itself naturally in the form to which Mr. Hill reduces, with so much skill, the equations depending on the first power of the eccentricity of the Moon's orbit.

"In solving this equation I fell upon the same infinite determinant as that considered by Mr. Hill, and I developed it in a similar manner in a series of powers and products of small quantities, the coefficient of each such term being given in a finite form.

"The terms of the fourth order in the determinant were thus obtained by me on December 26, 1868. I then laid aside the further investigation of this subject for a considerable time, but resumed it in 1874 and 1875; and on December 2 in the latter year I carried the approximation to the value of the determinant as far as terms of the twelfth order, or to the same extent as that which has been attained by Mr. Hill. I have also succeeded in reducing the determination of the inequalities of longitude and radius vector which involve the first power of the lunar eccentricity to the solution of a differential equation of the second order; but my method is much less elegant than that of Mr. Hill."

Adams adds that he has not had time to copy out and arrange the details of the calculations from his old papers, but that in the meantime he gives the result which he has obtained for the Moon's node, when the orbits of the Sun and Moon are supposed to have no eccentricity, and their mutual inclination is supposed to be indefinitely small. This consists of a *résumé* of the process by which his very accurate value of g was obtained.

Throughout his whole life Adams seems to have preferred to treat the lunar theory by means of its special problems in the manner which he has himself described in the words quoted above. This may be seen even in his solution of the problem of the secular acceleration as early as 1853. He had, however, the greatest possible admiration for Delaunay's general theory, which he regarded not only as most elegant and complete, but as likely to become the standard method of treating the lunar problem as a whole. It is interesting to notice how closely Mr. Hill's views, with respect to the importance of treating separately the special problems, accorded with those of Adams.

Among his other astronomical papers, particular reference should be made to his "Note on a remarkable property of the analytical expression for the constant term in the reciprocal of the Moon's radius vector" and "Note on the inequality in the Moon's latitude, which is due to the secular change of the plane of the ecliptic," which appeared in the 'Monthly Notices' for 1878 and 1881.

Although mathematical astronomy was the principal subject of Adams's life's work, still he found time to pay a good deal of attention to numerous branches of pure mathematics. This is apparent to anyone who reads through the questions set by him in the Smith's Prize Examination. It will be noticed also that these questions show a high degree of finish quite unusual in examination papers. His writings also exhibit throughout a degree of elegance, and what might be called "mathematical good taste," which indicate the importance that he attached to the form in which mathematical results should be expressed.

In 1878 he communicated to the Royal Society a paper in which the product of any two Legendrian coefficients was expressed as a series of Legendrian coefficients, and he was thus led to an interesting expression for the integral, taken between the limits ± 1, of the product of three Legendrian coefficients.

Like Euler and Gauss, he took very great pleasure in the numerical calculation of exact mathematical constants. We owe to him the calculation of thirty-one Bernoullian numbers, in addition to the first thirty-one that were previously known. The first fifteen are due to Euler, the next sixteen were calculated by Rothe, the whole thirty-one being given in vol. xx. of Crelle's Journal. Making use of Staudt's very curious theorem with respect to the fractional part of a Bernoullian number, Adams calculated all the numbers from B_{31} to B_{62}. The results were communicated to the British Association at the Plymouth Meeting in 1877, and were also published in vol. lxxxv. of Crelle's Journal. A much fuller account of the work, which was very considerable, appeared in an appendix to vol. xxii. of the 'Cambridge Observations,' where the process of calculation of the first, B_{32}, and of the last, B_{62}, is given in detail. Adams proved that if n be a prime number other than 2 or 3, then the numerator of the nth Bernoullian number is divisible by n. This afforded a good test of the accuracy of the work.

Having thus at his command the values of sixty-two Bernoullian numbers, he was tempted to make use of them by obtaining Euler's constant to a much greater extent than had before been practicable. Accordingly he calculated the value of this constant to no less than 263 decimal places. For this purpose it was necessary to obtain the values of a great many reciprocals and also of certain logarithms. He accordingly calculated the values of the logarithms of 2, 3, 5, and 7 to 263 (afterwards extended to 273) places. The value of the modulus of the common logarithms was also calculated to the same extent. The papers containing these results appeared in the 'Proceedings' of the Royal Society for 1878 and 1887. Anyone who has had experience of calculations extending to a great many decimal places is aware of the difficulty of manipulating with absolute accuracy the long lines of figures; but this was an enjoyment to Adams, and the work, as carried out with consummate care and neatness, in his beautiful figures, is an interesting memorial of the patience and skill that he devoted to any work upon which he was engaged.

It cannot fail to strike anyone who looks over the published papers of Adams that so few of them were produced quite spontaneously. In the majority of cases he was induced to give an account of some portion of an investigation of his own by the publication of a paper by some one else in which the same subject was treated.

For forty-five years he was constantly occupied with mathematical research, the extent of which is very inadequately indicated by the amount of his published writings. The lunar theory is the subject to which he paid most attention, treating it both according to his own principles and by Delaunay's method. He also devoted much time to the theory of the satellites of Jupiter and of Saturn. His accuracy of mind and power of thought enabled him to map out beforehand in his head the whole course of an investigation; and his mathematical instinct, combined with perfect familiarity with astronomical ideas and methods, guided him unerringly through the difficulties and dangers of the analytical details. He rarely put pen to paper until he had carefully thought out his subject, and when he proceeded to write out the investigation, he developed it without interruption. All his manuscripts are written so carefully, and in such clear and beautiful handwriting, that it is difficult to believe that they are not finished work which has been copied. He had no occasion for a waste-paper basket, scarcely ever destroying anything he had written, or performing rough calculations of any kind.

It is to be regretted that so much of his work should remain unpublished, and that his more considerable investigations can never appear now in the form which he would have desired for them. It would seem that he often delayed the immediate publication of his researches with the intention of improving points of detail which he believed to be capable of being exhibited in a better form. It is likely that he was sometimes carried on so far, as the subject opened before him, that he was unwilling to spare the time required to arrange so elaborate an investigation in a suitable form for the press; and it is probably safe to assume that, in all cases, his apparent reluctance to publish investigations which he was known to have carried out was due, either directly or indirectly, to the difficulty he experienced in satisfying himself with his work, having regard to his own standard of perfection and completeness. The syndics of the Cambridge University Press have undertaken the publication of his complete works, and it is believed that it will be possible to include most of the manuscripts in the form in which they have been left.

He was frequently asked to undertake calculations in connection with ancient eclipses or other astrononomical phenomena. These requests he never hesitated to comply with, although they made serious inroads upon his time. The willing aid thus freely given to others was characteristic of his generous nature.

Special reference must be made to his intimate acquaintance with the works of Newton, for whom he had the most enthusiastic admiration. Perhaps no one has devoted more careful and critical attention to Newton's mathematical writings, and especially the 'Principia.' When Lord Portsmouth presented to the University the large mass of papers which Newton left at his death, the arrangement and cataloguing of the mathematical portion of the collection was undertaken by Adams. It was a difficult and laborious task, extending over years, but one which greatly interested him, and upon which he spared no pains. He found that these papers threw light upon the remarkable extent to which Newton had carried the lunar theory, the method by which he had obtained his table of refractions (showing that the formula known as Bradley's was really due to Newton), and the manner in which he had determined the form of the solid of least resistance. In several instances he succeeded in tracing the methods that Newton must have used in order to obtain the numerical results which occurred in the papers. In 1887 he was asked by Trinity College to deliver an address in commemoration of the bicentenary of the publication of the 'Principia,' but unfortunately he was unable to accede to this request on account of the state of his health.

As Lowndean Professor he lectured during one term in each year, generally on the lunar theory, but sometimes on the theory of Jupiter's satellites or the figure of the Earth.

In 1861 Professor Challis resigned the directorship of the Cambridge Observatory, and Adams was appointed to succeed him. In 1870 a fine transit-circle by Simms was added to the equipment of the observatory. It was then decided to take part in carrying out the scheme planned by the 'Astronomische Gesellschaft' for the observation of all the fixed stars in the northern hemisphere down to the ninth magnitude. The zone which was assigned to the observatory was that lying between 25° and 30° of north declination. The work has been steadily carried on, nearly all the observations having been made by the first assistant Mr. Graham. The reductions are in a forward state, and the work will be completed and published within the next two years.

It may be mentioned that in an appendix to vol. xxii. of the 'Cambridge Observations,' Adams gave the formulæ and instructions which he had drawn up many years before for the formation of a proposed new fundamental catalogue, together with the mean places of the 84 fundamental stars from 1830 to 1870.

During his presidency of the Royal Astronomical Society in 1851-1853, the Gold Medal was awarded to Peters and to Dr. Hind. He served the office of President again in 1874-1876, when the

Medal was awarded to D'Arrest and to Leverrier. On each of these four occasions he delivered the address on presentation of the Medal.

In 1870, as Vice-President, he delivered the address on the presentation of the Medal to Delaunay. In 1866 he received the Medal of the Society for his contributions to the development of the lunar theory. The address, on this occasion, was delivered by Mr. Warren De la Rue. He received the Copley Medal of the Royal Society in 1848.

In 1881 he was offered the position of Astronomer Royal, which he declined. In 1884 he went to Washington as one of the delegates for Great Britain to the International Prime Meridian Conference.

He received the honorary degree of D.C.L. from Oxford, of LL.D. from Dublin and Edinburgh, and of Doctor in Science from Bologna and from his own university. He was a correspondent of the French Academy, of the Academy of Sciences of St. Petersburg, and of numerous other societies.

In the preceding paragraphs only the scientific side of Adams's life has been referred to; but it is not to be supposed that his thoughts and interests were confined to astronomy and mathematics. On the contrary his general reading was most extensive, and there were few subjects upon which he was not well informed. He also derived great pleasure from special pursuits, such as, for example, the study of early printed books, of which he formed a valuable collection. The accuracy of mind, to which reference has been made in connexion with his mathematical work was equally apparent in all other matters, and the exactness of his knowledge was no less remarkable than its range; in fact, as a man of science and of learning, his attainments were so finely blended that each added force to the other. His character was a singularly sympathetic and generous one, and probably in few men has there existed a more perfect balance between the moral and intellectual qualities. It has been truly said that he was really a great and, at the same time, a good man; and perhaps this notice may be most fittingly concluded by the following words, which were written by one who knew him well and attended him in his last illness *:—

"His earnest devotion to duty, his simplicity, his perfect selflessness, were to all who know his life at Cambridge a perpetual lesson, more eloquent than speech. From the time of his first great discovery, scientific honours were showered upon him, but they left him as they found him—modest, gentle, and sincere. Controversies raged for a time around his name, national and scientific rivalries were stirred up concerning his work and its reception, but he took no part in them, and would generously have yielded to other's claims more than his greatest contemporaries would allow to be just. With a single mind for pure knowledge he pursued his studies, here bringing a whole chaos into cosmic order, there vindicating the supremacy of a natural law beyond the

* **Dr. Donald MacAlister in 'The Eagle' for March, 1892.**

imagined limits of its operation; now tracing and abolishing errors that had crept into the calculations of the acknowledged masters of his craft, and now giving time and strength to resolving the self-made difficulties of a mere beginner, and all the time with so little thought of winning recognition or applause, that much of his most perfect work remained for long, or still remains, unpublished."

He was suddenly attacked by severe illness at the end of October, 1889, but he recovered sufficiently to resume his mathematical work in the usual way for several months. In June of the following year, he was again attacked by an illness from which he never completely recovered. He passed away on the early morning of January 21, 1892, after being confined to his bed for ten weeks, and was buried on January 26 in St. Giles's cemetery at Cambridge. There were many who thought that his last resting-place should have been in Westminster Abbey, but it is perhaps more fitting that he should lie in this quiet graveyard close to the observatory where so many years of his peaceful life were spent.

On February 20 a public meeting was held at St. John's College, Cambridge, with the view of taking steps to place a bust or other memorial of him in Westminster Abbey. The proceedings at this representative meeting bear testimony to the admiration and affection in which he was held by his friends, and to the widespread wish throughout the country for such a memorial to one who had contributed so much to science and had added lustre to his country.

The portrait which forms a frontispiece to this number is engraved from a photograph taken by Mrs. F. W. H. Myers on September 18, 1891, four months before his death.

J. W. L. Glaisher.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Shadows of Mimas and Enceladus.

Gentlemen,—

The shadow of Mimas having been seen by A. S. Williams, Esq., on March 20, on the central meridian of Saturn, at $10^h\ 40^m$ P.M. G.M.T., with a 6½-in. reflector (about 23^m before Mr. Marth's predicted time of inferior conjunction), it may be hoped that astronomers possessed of instruments *at least as large* will watch for the shadows of both Mimas and Enceladus. To assist them in this object I have drawn up, from Mr. Marth's Ephemeris, a table of the inferior conjunctions of Saturn's innermost satellites for April, limited to those which are visible at

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* A report of the meeting was published in a special number of the 'Cambridge University Reporter,' dated March 10, 1892.

Greenwich. The shadows will follow the satellites across the central meridian at short intervals; that of Mimas moves very swiftly.

MIMAS.

d	h	m	d	h	m	d	h	m
1	17	3	6	10	9	20	13	21
2	15	39	7	8	45	21	11	57
3	14	15	8	7	21	22	10	33
4	12	54	18	16	9	23	9	15
5	11	33	19	14	45	24	7	51

ENCELADUS.

d	h	m	d	h	m	d	h	m
3	13	21	10	9	42	21	8	51
6	7	9	14	12	24	25	11	27
7	16	0	18	15	3	29	14	6

In the case of *Mimas* the times of these conjunctions are very decidedly earlier than those which could be derived from the east elongations of the 'Companion to the Observatory.'

Faithfully yours,

Murston Rectory, Sittingbourne,
1892, Mar. 24.

A. FREEMAN.

Discovery of Comet b, *1892.*

GENTLEMEN,—

The evening of March 18 was very clear, and I swept the region of Cepheus with my 10-inch reflector, power 40, field 65′. At about $8^{h}\ 30^{m}$ I encountered a small, faint nebulosity situated 3° E.N.E. of δ Cephei, and at once suspected the cometary nature of the object, as I had never picked it up before. Motion soon became apparent, and its daily rate, as derived from observations at $8^{h}\ 30^{m}$ and $11^{h}\ 45^{m}$, was about 50′ eastwards. The position of the comet was estimated at R.A. $22^{h}\ 42^{m}$, Dec. +59°.

I reobserved it on March 19, 21, 22, 23, 24, and 28, and on the three latter nights its approximate places were:—

1892.	G.M.T.	R.A.	Dec.
March 23	$9^{h}\ 30^{m}$	$23^{h}\ 11^{m}\ 22^{s}$	+59° 57′
24	9 10	23 17 36	+60 5
28	10 10	23 42 30	+60 29

On March 22 and 23 the comet was in the field with the fan-shaped cluster No. 7510 N.G.C., and the two objects must have been in conjunction on March 22, 16^{h}, the cluster being 7′ north at the time.

The comet will be close to 12 Cassiopeiæ on April 3, and will pass near γ Cassiopeiæ on April 8. It is a very inconspicuous object, and has a nucleus of about the 13th magnitude.

Yours faithfully,

Bristol, 1892, March 29.

W. F. DENNING.

Transit of the Shadow of Mimas.

GENTLEMEN,—

Transits of the shadow of Titan over the disc of Saturn have been frequently observed, but observations of the shadows of the other satellites seem to have been rarely, if ever, recorded previous to the present apparition of the planet. Within the last few months, however, the shadows of the three satellites Rhea, Dione, and Tethys have been seen, either by the Rev. A. Freeman, with a 6½-in. Grubb refractor, or by the writer, and doubtless their transits will have been frequently watched by others. I have now succeeded in satisfactorily observing the transit of the shadow of the innermost satellite Mimas.

The night of March 20 was an unusually fine one, definition being very sharp and fairly steady. The shadow of Mimas was seen, almost directly on looking through the telescope, as a minute dusky speck just on the southern edge of the obscure streak formed by the rings across the face of the planet, and with which it was in apparent contact. The shadow did not appear perfectly black, but was dusky. A very short view showed that it was moving rapidly westward, and the following observations were made of its transit over the central meridian of the disc:—

Mar. 22,	$10^{\mathrm{h}}\ 36^{\mathrm{m}}{\cdot}5$	G.M.T.	Shadow nearly in transit.
	10 40	„	Up; pretty exact.
	10 44	„	Clearly past.

The transit was observed with a power of 420 on my 6½-in. Calver reflector, with which power the image was very sharp and distinct. The shadow was also seen distinctly with powers of 320 and 225. Its motion westward was very rapid and striking, there being a marked displacement of position in the course of a very few minutes. The shadow was watched until $11^{\mathrm{h}}\ 20^{\mathrm{m}}$, at which time it was approaching the preceding limb of the planet.

According to Mr. Marth's invaluable 'Ephemerides of the Satellites of Saturn,' Mimas was in conjunction with the following end of the ring at about $8^{\mathrm{h}}{\cdot}1$, and with the preceding end at $14^{\mathrm{h}}{\cdot}0$. The satellite should therefore have been in mid-transit at about $11^{\mathrm{h}}{\cdot}05$, and, it being only 4 days after opposition, the shadow should have transitted nearly at the same time. My observation shows therefore that the satellite is at present about 20^{m} in advance of its ephemeris place.

In conclusion, I would call attention to the very great importance of observing as many transits as possible of the shadow of Mimas during the remainder of the present apparition. The position of the satellite has hitherto been usually obtained either by observing its conjunctions with the ends of the ring, or by micrometer measures connecting it with the ball, the faintness of the satellite generally necessitating such measures being made when it is not very remote from its elongations. The orbit of this satellite is so constricted, that in either case the apparent motion of the satellite is greatly diminished, owing to the fact that a large proportion of the real motion takes place in a direction from or towards the observer, and hence the uncertainty of the determination is thus largely increased. But in observing the transit of the shadow, not only is the apparent motion at its greatest, but the instant when the disc is bisected can be determined with great exactness; and the operation is, moreover, a very simple one. In a telescope of the size of mine, it is probably only on exceptionally favourable occasions that the shadow of this satellite can be seen. But with an instrument of 12-in. or larger aperture, of which there are so many now in use, the transits should be observable on any fairly good night. At present the shadow is projected upon the bright equatorial zone, a circumstance favourable to its observation. In future years it will probably fall on the more obscure ground to the south, and it will probably be then much more difficult to detect.

Burgess Hill,
1892, Mar. 22.

Yours faithfully,
A. Stanley Williams.

The Birthplace of Horrox.

Gentlemen,—

I have been informed by Mr. R. C. Hall, of Sandringham Drive, Toxteth Park, Liverpool, that the etching accompanying the account of Horrox in the 'Palatine Note-book' for December 1882, which is reproduced in the sixth volume of the 'Observatory,' and is stated to be a view of that astronomer's birthplace, is founded on a mistake. As is stated in the article in question, the farmhouse in which Horrox was born was pulled down in 1863 when the Garston and Liverpool Railway was made, the site being very near where the Otterspool Station now stands. But the small cottage, of which a view is given in the etching referred to, is *still standing*.

Mr. R. C. Hall has been collecting some interesting biographical particulars respecting Horrox, which he has narrated in two lectures delivered in Liverpool. These he hopes shortly to print, and to give with the publication a view, copied from a drawing taken by a friend who resides near Otterspool, of the real birthplace of Horrox, which was removed, as above mentioned, nearly thirty years ago.

Yours faithfully,
W. T. Lynn.

Blackheath, 1892, Mar. 23.

Probable Variability of α Ceti.

Gentlemen,—

In the earliest paper ever sent to the Royal Society by Sir William Herschel (it was communicated by Dr. Watson, Jun., of Bath, in 1780) he remarks that the relative brightness of α and β Ceti must have changed since the time of Bayer, as β was then so much brighter than α that he could not have lettered them in that manner had they been the same when he drew up his 'Uranometry.' Now some time ago I called attention in the 'Observatory' to the fact that Bayer, though he made no attempt (as many have hastily supposed) to arrange the letters in the order of the stars' magnitudes in the different constellations, did really endeavour to mark the brightest star in each by the letter α. But in addition to this, we find that both Flamsteed and Hevelius register α Ceti as a magnitude brighter than β, which makes it very probable that that was the order in the seventeenth century, and that α had undergone a temporary increase of brightness. Tycho registered both stars of the second magnitude, and possibly both may be variable. In the 'Almagest' both are recorded of the third magnitude. Bradley marks both as 2·3, and we all know that β is now (as in Herschel's time) much brighter than α. Now that photometry enables us to determine magnitudes more exactly, it will be interesting to note whether any changes are shown in these stars.

Yours faithfully,

Blackheath, 1892, Mar. 7. W. T. LYNN.

Boeddicker's Drawings of the Milky Way.

GENTLEMEN,—

One of the subjects alluded to in your review of this work is not so simple as the reviewer seems to think. It may be very desirable that words describing nebulosity &c. should be confined to definite meanings, but have we arrived at doing so as yet? Perhaps we ought to distinguish between a nebula and a cluster solely on spectroscopic grounds, but it must be remembered that up to the present we have no spectroscopic evidence of the nature of by far the larger proportion of the supposed nebulæ in the sky. Would the reviewer therefore suggest some new word to signify matter whose nature we are doubtful of? For the words "nebula" and "nebulosity" have been continually used hitherto to describe matter of such doubtful character. It is quite true that in the case of the Milky Way there is no doubt that most of the light comes from stars, and I suppose the reviewer holds that true nebulous matter—using the word "nebulous" in the restricted meaning given by him—forms a quite insignificant proportion of the whole. But do we know that this is the case? Wolf's photographs of Cygnus seem to show that no inconsiderable part of the light is what is termed nebulous, although as yet we have no evidence of its nature; and it may be that a much larger proportion than has been thought is of the same kind. I have pointed out in my 'Structure of the Sidereal Universe,' p. 19, the uncertainty in the use of the words while the nature of so-called nebulosity is still unascertained.

With regard to the suggestion that it would have been better to have used opera-glasses rather than have relied on the naked eye in drawing the Milky Way, I would point out that while such a delineation would be very useful, the results of the two modes of observation would be very different, even if the glasses were of quite low power; and if more powerful field-glasses were used, my observations contained in the 'Structure of the Sidereal Universe' indicate what an enormous amount of detail would then be seen, and what a very much greater work it would be to make a complete drawing of the Milky Way with such glasses.

I agree with the reviewer that Dr. Boeddicker should have described any peculiarities of his own eyes, but perhaps we may assume, in the absence of any such reference, that they are as normal as possible.

Yours faithfully,

Sunderland, 1892, Mar. 8. T. W. Backhouse.

Clavius.

Gentlemen,—

I would like to call the attention of observers to the great curved chain of ring-plains which extends across the lunar walled-plain of Clavius, beginning with the large ring-plain Clavius *a* and ending with the very small ring-plain or rather crater halfway between the ring-plains Clavius *i* and *k*, and a trifle to the right of the line joining their centres (not shown by Neison, but traceable in the Lick Observatory photographs). These ring-plains all seem to bear a striking family resemblance to each other, the only difference being one of size, which decreases in regular progression from the first to the last. I lately had an opportunity of examining them with a 3¼-in telescope and was much struck by this resemblance. Would not further study with larger instruments *provc* them to belong to one and the same type of formation? Once prove this, and it follows that Tycho, from its resemblance to Clavius *a*, is only the smallest of them over again on a much grander scale; and if Tycho, why not Copernicus? Neison mentions that D, the second in size, has a central mountain; may they not all have central mountains? And is not the smallest of them of the same type as those formations usually termed craters, such as Ptolemaus A and Sosigenes *a*? We have an excellent chance to prove the largest lunar formations of the same type as the smallest; it should be improved.

I also wish to call attention to some dark streaks on the bright floor of the walled-plain Wilhelm I. On March 8, 1892 (12^h P.S.T.), I discovered with my 2-inch spy-glass and a power of 64 two such streaks dividing the floor approximately into thirds. Southern streak points to south wall of Tycho; northern points to Tycho A. Breadth of each streak was roughly estimated as equal to one eighth the interior diameter of Wilhelm I. on a line perpendicular to the southern streak. A comparison of Neison's Map XVII. with any good photograph of this region will show his representation of the features immediately around Wilhelm I. to be very faulty.

Yours truly,

Berkeley, California, 1892, Mar. 10. Roger Sprague.

OBSERVATORIES.

Edinburgh (Royal Observatory).—Dr. Copeland, the Astronomer Royal for Scotland, states in a private letter that "Nova Aurigæ is declining at the rate of $0^{m}\cdot2$ to $0^{m}\cdot3$ per day with remarkable steadiness. Its spectrum seems to have undergone no material change."

Many of our readers may have seen the short account in the newspapers of the fire at Dr. Copeland's private house in Edinburgh; we give in his own words an account of the fire, which shows the fortunate escape of himself and his family from a very great danger, due in a great measure to his coolness and presence of mind:—

"The fire was detected about 4.30 A.M. on the 8th March, but it must then have been burning fiercely for some time, as it had already obtained a firm hold of the south-west corner of the house from the basement to the roof—five stories in all. The wooden lining of the lift contributed in a high degree to the spread of the fire. By the time that the inmates of the upper part of the house, six in number, had collected in the communicating front rooms of the second floor, the smoke had become so suffocatingly dense that it appeared in the highest degree inadvisable to attempt a rush down the long stone staircase. At the same time, the fire was spreading so rapidly overhead and in the back of the house that it was impossible to say at what moment it might render our position untenable. We therefore decided to descend to the balcony of one of the first-floor windows by the aid of sheets knotted together. While the rope was being prepared and overhauled, a policeman with some difficulty reached the balcony from the next house. Five of the party were lowered to the balcony without difficulty. Just after this the firemen arrived and opened the lower part of the house, thus freeing the stairs from smoke, and rendering them practicable. A considerable quantity of chemicals and laboratory appliances were altogether lost. A good many books are more or less disfigured by water, fire, and smoke, but happily all the finer parts of the library are absolutely intact. The damage to the house, including the total destruction of the roof, may be taken at about £1000, while my own effects, apart from the loss of notes and papers, have suffered to fully one-third of that amount, but this is covered by insurance. Although it was so cold that the spray from the fire-engine froze on the pavement and even in the beards of the firemen, we all escaped without taking cold, thanks, doubtless, to the thoughtful care and attentions of our kind neighbours. It is highly probable that the fire originated in the lower part of the lift, which was separated only by a thin brick wall from the flue of the wash-house, which had been in use on the previous day."

Madras.—Another volume of the series of meridian observations has been published, containing the results for 1871, 1872, and 1873.

It is similar to the volumes already noticed (see 'Observatory,' Nos. 125, 145, and 173), and leaves four more of the series to come. The present volume has been passed through the press by Mr. C. Michie Smith, who since the death of Mr. Pogson has been "Officiating Government Astronomer." We have not examined the body of the work very carefully, but there is evidence in the few words of Introduction that Mr. Smith might be more careful in correcting his proofs. "Auwer's" may be a printer's error; but surely "Robert Norman Pogson" ought not to have been passed by one so intimate with him. We gather that the 'Atlas of Telescopic Variable Stars' has been entrusted to Mr. Pogson's nephew.

NOTES.

NOVA AURIGÆ.—In the notes on this star, rather hurriedly drawn up last month, an unfortunate blunder was made, which has been pointed out by several correspondents. On p. 142, line 20, for "approach" read "recession," and in the next line for "recession" read "approach." The sentence as it stands is directly at variance with Dr. Huggins's remarks which follow.

Of additional information, perhaps the most interesting paper is Prof. E. C. Pickering's contribution to the 'Astr. Nachrichten,' No. 3079, the substance of which is as follows:—

"The photographs of the Henry Draper Memorial afford a means of studying the past condition of the portion of the sky in which the new star in Auriga has recently been discovered. Eighteen photographs of this region were taken with the 8-inch photographic telescopes, from November 3, 1885, to November 2, 1891. On none of them was the star visible, although on all but five, stars of the eleventh magnitude were shown, and on some of them stars of the thirteenth magnitude were visible. On the plate taken November 2 spectra of stars of the eleventh magnitude were shown. It is probable that the star was not visible during these six years. Five plates, on the other hand, taken from December 16, 1891, to January 31, 1892, show stars of the twelfth magnitude, and the Nova appears as a bright star of the fifth magnitude. A still closer watch of the sky has been kept for the past year with an instrument intended to furnish the scale of stellar magnitudes of the brighter stars to which the stars photographed with the Bruce 24-inch photographic telescope will be referred. This instrument is called the transit photometer. It consists of a telescope having a Voigtländer doublet of five inches aperture as an objective mounted like a transit instrument and driven automatically by clockwork in such a way that a region extending from 30° below the north pole to —30° in declination and in right ascension over three hours will be photographed on one plate. The exposure given to each portion of this region is two seconds, and in this time stars of the sixth magnitude and brighter are distinctly shown. Plates were thus obtained on thirteen nights, from Oct. 21 to Dec. 1, 1891. On none of these was the star visible, although χ Aurigæ, magnitude $5^{\mathrm{m}}{\cdot}00$, was always clearly shown. On twelve nights, beginning Dec. 10, 1891, and ending on Jan. 20, 1892, similar plates were obtained, on all of which the new star was clearly seen. Measurements of these images were made by Mrs. M. Fleming, who also made the examination of the photographs described above. The Nova was compared directly with χ Aurigæ, and the difference in brightness estimated in grades, as in Argelander's method of observing variable stars. To secure independence in the measurements an assistant recorded the numbers of the plates and the measures, rearranged their order according to chance, and returned them to Mrs. Fleming to be measured again. A third measure was made in the same way. On Dec. 10, 11, 18, 28, and 30, 1891, and on January 5, 8, 9, and 16, two photographic images of this star were obtained. On Dec. 17 and 18 the first measures were repeated. The corresponding dates and magnitudes are given in the following table:—

Date.	Mag.	Date.	Mag.
1891 Dec. 10..	$5^{\mathrm{m}}{\cdot}37$	1891 Dec. 30..	$4^{\mathrm{m}}{\cdot}60$
11..	5 ·33	1892 Jan. 5..	4 ·58
13..	5 ·22	8..	4 ·72
17..	4 ·67	9..	4 ·67
18..	4 ·46	16..	4 ·96
28..	4 ·55	20..	5 ·23

"The accordance of the measures is shown by the average residual which equals $\pm 0^{\mathrm{m}}{\cdot}05$. From this it appears that the star was fainter than the eleventh magnitude on November 2, 1891, than the sixth magnitude on December 1, and that it was increasing rapidly on December 10. A graphical construction indicates that it had probably attained the seventh magnitude within a day or two of December 2, and the sixth magnitude December 7. The brightness increased rapidly until December 18, attaining its maximum about December 20, when its magnitude was $4^{\mathrm{m}}{\cdot}4$. It then began to decrease slowly with slight fluctuations until January 20, when it was somewhat below the fifth magnitude. All of those changes took place before its discovery, so that it escaped observation for nearly two months. During half of this time it was probably brighter than the fifth magnitude.

"Since the announcement on February 2 of its discovery it has been closely followed at this observatory, both visually and photographically. Comparisons by Argelander's method visually are made every clear night by Mr. O. C. Wendell and Mr. W. M. Reed. Comparison stars have been selected so as to form a sequence, having intervals of three- or four-tenths of a magnitude.

"The magnitudes of all these stars will be determined photometrically for use in the final reduction. On every clear night Mr. Wendell also compares the Nova with the star *s* in the list given above. These observations are made with the 15-in. equatorial by means of a polarization photometer (H.C.O. Annals XI.

Part I). Measures of the Nova and of the brighter comparison stars are made by Mr. S. J. Bailey and the writer with the meridian photometer. Five independent series of visual observations will thus be obtained which will test the reality of any apparent fluctuation.

"Photographic charts are made every clear evening with the 8-in. Draper telescope, and photographs are also taken with the transit photometer.

"An important series of photographs of the spectrum is being obtained with the 11-in. Draper telescope, and since the star will probably soon be too faint to photograph in this way, spectra are also taken with the 8-in. telescope, using a small dispersion. The spectrum can thus be photographed until the star is as faint as the eleventh magnitude. With these last photographs several lines, including *F*, *G*, *h*, *H*, *K*, and *a*, appear to be bright. But on closer examination they are shown to be really dark, with the edge of greater wave-length bright. This is confirmed with the greater dispersion, the bright lines really consisting of broad bands sharply defined on the edge of smaller wave-length. The breadth of the bands is not due to poor definition, since numerous fine lines are also visible. Many of the lines, including the *K* line and those due to hydrogen, are double. The evidence that this doubling is due to the different velocities of different portions of this object is not conclusive, owing to the breadth of the bands. The difference in velocity indicated by the separation of the lines is about 370 kilometres per second. The apparent separation of the different lines in the photograph increases as the length diminishes, as it should, according to this theory, owing to the increasing dispersion of the violet rays.

"Two explanations have been offered of the sudden increase in light of stars of this class, the mechanical theory that it is caused by approach or collision, and the chemical theory that it is due to volcanic action. The doubling of the lines strengthens the first of the above theories rather than the second."

Prof. C. A. Young, of Princetown, has succeeded in determining the positions of 12 bright lines in the spectrum as follows:—

4340 ($H\gamma$); 449; 4861 (F); 4922; 5015; 5165; 5260; 5304; 559; 590 (D?); 632; 6563 (C).

He also glimpsed a faint line below C on February 6, and another above G,—in all probability *h* ($H\delta$). "The lines were not *persiennes*, but true lines, though rather diffuse, as if formed under pressure, C and F especially."

The Nova is now very faint, having decreased in brightness after March 9 at the rate of nearly half a magnitude per day.

COMET NOTES.—A bright comet, visible to the naked eye, was discovered by Swift on March 6. Observations were obtained at Cape Town and Boston on March 8 and 9, and from these Prof. Kreutz deduces the following ephemeris:—

	R.A.			S. Decl.	
	h	m	s	°	′
April 2	20	48	19	7	22
6	21	1	37	3	41
10		14	16	0	3

Perihelion passage is on March 26.

Later observations show that the comet is about 5^{m} east and 2° north of these positions.

A faint comet was discovered by Denning on March 18 and was observed at Vienna on March 19, 11^{h} 30^{m} Vienna M.T., in R.A. 22^{h} 46^{m} $47^{s}\cdot13$, N.P.D. 30° 47′ 17″. The motion is north preceding. Denning reobserved it on March 21^{d} 13^{h}, in R.A. 23^{h} 0^{m}, N.P.D. 30° 18′, close to the cluster N.G.C. 7510.

Ephemeris for April.

	R. A.			N. Decl.			R. A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
Apr. 2	0	13	50	60	39	Apr. 10	1	1	37	60	18
6	0	38	8	60	34	14	1	24	4	59	52

Winnecke's Comet was picked up (by whom is not stated) on March 18, 10^{h} $47^{m}\cdot3$ Vienna Mean Time, in R.A. 12^{h} 43^{m} $27^{s}\cdot47$, N.P.D. 59° 24′ 22″. The predicted place in Dr. Haerdtl's ephemeris was 12^{h} 43^{m} $29^{s}\cdot41$, N.P.D. 59° 34′ 8″.

We continue his ephemeris for April with a correction of $+2^{s}$ to the R.A. and $-10'$ to the declination.

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
Apr. 6....	12	17	54	38	13	Apr. 18....	11	56	6	41	50
10....		10	53	39	35	22....		48	36	42	43
14....		3	33	40	47						

The last two places are approximate; Haerdtl's ephemeris ends on April 15.

Continuation of the search ephemeris of Tempel's Comet:

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
Apr. 10....	18	6	4	21	18	Apr. 25....	18	19	24	22	41
15....		11	11	21	44	30....		24	33	23	12
20....		15	39	22	11	May 5....		25	53	23	46

There is a probable error of 20^{m} in R.A., 1½° in Decl.

ALGOL.—Mr. S. C. Chandler declares that Algol is not only a double but a triple body. "The inequalities in the period of this star, and also some other phenomena, are satisfactorily accounted for by the following theory.

"*Algol*, together with the close companion whose revolution in 2^{d} 20^{h} 8^{m} produces by eclipse the observed fluctuations in light, according to the well-known hypothesis of GOODRICKE, confirmed by the elegant investigation of VOGEL, is subject to still another orbital motion of a quite different kind. Both have a common revolution about a third body, a large, distant, and dark companion or primary, in a period of about 150 years. The size of this orbit around the common centre of gravity is about equal to that of *Uranus* round the Sun. The plane of the orbit is inclined about 20° to our line of vision. *Algol* transitted the plane passing through the C.G. perpendicular to this line of vision, in 1804 going outwards, and in 1869 coming inwards. Calling the first point the ascending node, its position, reckoned in the ordinary way, is about 65°. The orbit is sensibly circular, or of very moderate eccentricity. The longest diameter of the projected ellipse, measured on the sky, is about $2''\cdot7$." A necessary consequence of this theory is an irregularity of proper motion with an amplitude of something over $0^s\cdot2$ in R.A. and nearly $1''\cdot4$ in declination. This theory Mr. Chandler supports by the photometric observations, and also the observations of position, from Bradley's time to the present, making out a very good case for the irregularity in proper motion claimed.

Other variables of this type also show irregularities in their light curve, which may be due to similar causes; and it is therefore urgently to be desired that the positions of the stars should be carefully watched. Mr. Chandler would like to see heliometers brought to bear on these stars, and especially on γ Cygni, which has a cyclical fluctuation of about 2½ years; and λ Tauri is also a favourable instance. For details we must refer our readers to 'Astronomical Journal,' Nos. 255 & 256.

SOUTHERN STARS WITH LARGE PROPER MOTIONS.—Prof. Lewis Boss gives a list of 27 such in the 'Astronomical Journal,' No. 254. He is preparing a catalogue of large proper motions and has already 2700 stars; but the collection is weak as regards the Southern Hemisphere. Prof. Boss would like to see new reductions of the Paramatta Catalogue (by differential processes as suggested by Mr. Stone), of Taylor's Madras Observations, and of Piazzi's observations, in the near future. He is "strongly of opinion that the accomplishment of these three works would prove a wise expenditure of labour, and that it would place the determination of proper motion for southern stars, at least, upon a vastly improved basis."

ORBIT OF IAPETUS.—Prof. Asaph Hall recently made a series of observations on Iapetus complementary to a series made in 1875, 1876, 1877, In both cases the difference of declination between Saturn and Iapetus was very small, but the apparent motion of the satellite was in opposite directions. In such cases the determination of the mean distance of the satellite from the primary depends almost wholly on differences of right ascension. The value of Saturn's mass deduced in 1875–7 was $\frac{1}{3481}$. From the modern observations Prof. Hall obtains the value

$$\frac{1}{3485\cdot7 + 1\cdot28}.$$

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 188. MAY. 1892.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, April 8, 1892.

E. J. Stone, M.A., F.R.S., *Vice-President* (Past President), in the Chair.

Secretaries: H. H. Turner, M.A., B.Sc., and E. W. Maunder.

The Minutes of the previous Meeting were read and confirmed.

Mr. Maunder. Sixty-one presents have been received since the date of the last Meeting. One calling for special mention is a portfolio of views and plans of the Nice Observatory, a very beautiful series, showing every detail of the buildings, presented by M. Bischoffsheim.

A vote of thanks was passed to the donors of the presents.

Mr. Turner read a paper "On the Spectrum of the Great Sun-Spot Group of 1892 February," by *Prof. K. D. Naegamvala, M.A.* On February 12, while examining the spectrum of this group, I found that besides the unusual thickening, the F and C lines were reversed at the centres of the two chief nuclei of the group. On the next day, at 10.30 Madras mean time (5.10 G.M.T.), this was much increased, the C line gradually extended from one nucleus to the other, and was displaced towards the more refrangible side by about its own thickness. The F line was similarly affected, a small *dark* prominence being observed on the F line towards the less refrangible side. The existence of this was confirmed with the C line. In addition to the C and F lines, the G line, the D_3 line, the D (sodium) lines, and all the "*b*" lines were found reversed. A strong absorption line was seen at D_3, and this was not of uniform thickness. At about 10 A.M., G.M.T., the disturbance had almost ceased, the D_3 line was completely absent either as an absorption or emission line, and the only line seen reversed was C.

We have also a paper by *Mr. S. W. Burnham* on the double star Σ 3128, in which he gives measures of this star, and calls attention to the scarcity of these, undoubtedly due to the fact that at all times the star is difficult to reach. It seemed desirable that this star should be measured every three years.

We have also a paper entitled "Note on the Lunar Theory," by *Ernest W. Brown, M.A.* The paper confirms some of the results obtained by Dr. Hill, and concludes, "I hope before long to publish the complete determination of all the inequalities depending on the eccentricity and mean motions, including that part of the motion of the perigee which depends on the eccentricity of the Moon." It is a short paper, and gives no very great results, but its importance and significance lies in the fact that at last there is a young Englishman giving his attention to the Lunar Theory.

Mr. Turner read a "Note on the Transit of Titan, March 11, 1892," by *Arthur Mee.* This evening, with pretty fair definition, at about 10.15 I turned my 8½-inch Calver equatorial on Saturn. I immediately saw, a little within the southern limb and slightly east of the meridian, a dark spot, and almost immediately afterwards a brown spot still further to the east. I watched the pair with powers 200–400, till I had to leave the telescope. The dark spot seemed central at 10.30. Looking up the almanac next day, I found the dark spot was the shadow of Titan in transit, and it at once occurred to me that the brown spot must be the satellite itself, and as I can find no record of a similar previous observation, I venture to forward this note, hoping that others with ampler opportunities will tell us more of a deeply interesting if not unique phenomenon.

This paper is chiefly remarkable because several Fellows have made similar observations, and the conditions are now favourable for seeing the shadows of satellites on Saturn.

Mr. Maunder. Mr. Freeman observed the transit of Titan and its shadow on the same night (1892 March 11) with a 6½-inch telescope, and Mr. Isaac Ward observed the shadow of Titan during the transit of March 27, with a telescope of 4·3 inches. March 11 was before the opposition of Saturn, and March 27 after it, so that in one case the shadow preceded the satellite, and in the other case followed the satellite on the body of the planet.

Mr. Downing. It is worth pointing out that in the last few months several observers have been able to record observations of the satellites of Saturn, with comparatively small apertures. Mr. Freeman has only a 6½-inch aperture, and has made a long series of observations of the satellites with this. There is no reason why the satellites of Saturn should not be as much observed as those of Jupiter, and such observations if continuous will be extremely useful to those who have to compute the ephemeris of position and to study the perturbations of the satellites.

Mr. Maunder. I think Mr. Ward used 4⅓ inches and Mr. Freeman 6½ inches aperture.

Dr. Spitta. How far did Mr. Freeman see the shadow go across? Did he observe the time it was on the central meridian or estimate it?

Mr. Maunder. He estimated it from his observations.

Dr. Spitta. The interesting point is as regards the observations of contact.

Mr. Maunder. Precisely. I do not know whether he saw it come upon the limb.

Mr. Ingall. A telescope of 6 inches aperture would have the advantage on ordinary nights over a larger one, except in the case of Mimas, which is very small. I recollect when I used a 5-inch telescope instead of the 10-inch I now have that with the former it was a more ordinary circumstance for me to see planetary detail than with the 10-inch.

The Chairman. Perhaps your 5-inch was a better telescope than your 10-inch.

Mr. Ingall. I made them both myself and I think not. It is the air which affects the larger aperture most.

Mr. Brett. There is no reflection upon the quality of the telescope, because the smaller the aperture the more often you can use it.

Mr. Turner. Three papers have been received on the new star in Auriga. Mr. Burnham's observations were confined to micrometric measures of the new star and surrounding stars, and in the course of his observations he rather characteristically discovers two or three new double stars. 26 Aurigæ is amongst these, a separation of 0″·15 being found.

Mr. Knott read a paper on "The New Star in Auriga." The observations extend from March 12 to April 3, and are eleven in number. The magnitude of the star has during this time fallen from 7 on March 12, to 13·5 on April 2 and 3. As the star decreased in magnitude it deepened in colour to a ruddy orange. I give also a list of approximate places for 1892, for six stars near the new star, together with estimations of their magnitudes, which range from 10·6 to 13·6. Unfavourable weather and the presence of the Moon have at times made observations difficult.

Dr. Spitta. Were the observations made with the refractor or the reflector?

Mr. Knott. The Alvan Clark refractor of 7⅓ inches aperture.

The Chairman. We have a paper on some observations of the new star in Auriga at the Radcliffe Observatory, Oxford, extending from Feb. 3 to March 31, but since the latter date we have not been able to distinctly recognize the star. It is now practically invisible with our aperture, but whether owing to moonlight or haze I cannot say. Five observations were made with the transit circle between Feb. 3 and 18, and a naked eye estimation made on Feb. 16. The increase in magnitude about Feb. 18 is clearly shown. Since Feb. 24 the observations have been made with the Barclay equatoreal and show a steady diminution in brightness from 6·1 on Feb. 24 to 14· on March 31. The comparison stars used and adopted magnitudes for these are given together with a chart showing the Nova and surrounding stars.

Mr. Turner. I believe Mr. Criswick, at Greenwich, finds that the star is not shown on a photograph with 27 minutes exposure with the 13-inch charting telescope.

Votes of thanks were accorded to the authors of the various papers.

Captain Abney read his paper "On Estimation of Star-magnitudes by extinction with the Wedge."

Some experiments which I have recently made and communicated to the Royal Society in a paper by Gen. Festing and myself, "Colour Photometry, part iii.," have a direct bearing on the estimation of star magnitudes by the wedge, and it has seemed advisable to put on record in what way it occurs.

Amongst other matters the question arose as to the amount by which the intensity of any ray of the spectrum would have to be reduced before it became invisible. Of course the comparative luminosity of the spectrum had to be known in all its various parts, and where the absolute luminosity in candle-power of any one part was known, the others could be calculated. In the experiments in question, the spectrum used was that formed by the positive pole of the electric light, and the comparative luminosities were measured, as also the absolute intensity of the light at D coming through a slit placed in the spectrum. This light was spread out into a special patch by a suitable optical arrangement to fall on a screen at the end of a darkened box, where a black screen with a white disk received it. The light was gradually diminished until the eye, which observed through a small aperture in the box, could no longer distinguish the white disk. Measures taken in this way showed that if the D light were reduced to $\frac{350}{10,000,000}$ of a standard amyl lamp, the illumination was so feeble that the white disk could no longer be seen, and no scintilla of light was visible in the box. The green light E had to be reduced to $\frac{65}{10,000,000}$, the F light to $\frac{150}{10,000,000}$, the G light to $\frac{3000}{10,000,000}$, whilst the red light had only to be reduced to $\frac{110,000}{10,000,000}$ before the screen was invisible. These were the numbers when the D light in the original spectrum had a luminosity or brightness equal to one amyl lamp illuminating a screen one foot off. If we make the rays of the spectrum throughout equal to one amyl lamp, these numbers of course would be modified. The D light would remain at $\frac{3500}{10,000,000}$, since it was originally of the value of one amyl lamp, but the E light would be $\frac{35}{10,000,000}$, the F light would become $\frac{17}{10,000,000}$, and the G light $\frac{15}{10,000,000}$, while the C light would be $\frac{22,000}{10,000,000}$.

Further, it was found that near F, but on the G side of it, the extinction value of one amyl lamp became $\frac{15}{10,000,000}$, and remained the same to the extreme violet. It will thus be apparent that to extinguish two lights of equal luminosity, one of the C colour and the other of the G, the reduction to produce extinction required for the latter is nearly 1500 times greater than that required for the other. I may remark that a little further than C, towards the extreme red, the extinction is the same for every ray when equal luminosities are used.

Captain Abney illustrated this by means of curves projected by the lantern.

The Young Helmholtz theory of colour vision tells us there are three fundamental sensations, which are popularly named red, green, and violet, and that at each end of the spectrum but one sensation is excited, viz. at the extreme red the red sensation, at the extreme violet the violet sensation, and that intermediate colours are produced by different degrees of stimulation of the three sensations. The straight lines at the ends of the extinction curves for equal luminosities confirm this. But the violet sensation is 1500 times more persistent than the red, and probably 25 times more than the green; hence when we extinguish blue-green, which is probably a mixture of all three, it will be the violet sensation which be last extinguished. With the green 25 times, and the red 1500 times more strongly excited than the violet, they would be extinguished together. Measures show that in a mixture of 1 part of pure violet with 1500 parts of pure red, the violet is unperceived, and that 1 part of violet in 25 green will scarcely have any effect. We may take it, therefore, that up to near D, where according to the Young Helmholtz theory the violet sensation ceases, the sensation that is extinguished is the violet sensation. The reciprocals of the extinction curve of the spectrum will therefore give the amount of violet sensation which exists between the extreme violet and near D. This curve of reciprocals is called, therefore, the persistency curve.

Everybody is not gifted with the same faculty for observation of colour; in some cases red and blue, and in other cases violet and green, are seen as shades of the same colour.

I have plotted the curves for several of such colour-blind persons, and have found two cases in which monochromatic vision existed; to these all colours appeared as shades of grey. I found the persistency curve of these persons coincided with the persistency curve of my own eyes, which are normal, and also that the persistency curves of red-colour blind and green-colour blind persons are the same, while a violet-colour blind person is totally different. These curves were also projected by the lantern.

The first sensation to be excited by a feeble light is the violet, and in the electric light spectrum the maximum of the curve of this sensation is close to E, coinciding with the luminosity curve.

The wedge therefore has to reduce the light of the spectrum so that the violet sensation is not excited; and the last ray to be extinguished will be the green near E, that is supposing the light to be similar in quality to that of the positive pole of the electric arc, which is very similar to the light of the Sun on an April day. In extinguishing star light with a wedge, we are not extinguishing its spectrum, but the violet sensation contained in its light.

In a paper in the 'Philosophical Transactions of the Royal Society' on the "Transmission of Solar Light through the Atmosphere," I have shown that the total absorption of light by a medium of varying thickness can be ascertained by measuring the absorption of a single ray which lies close to the maximum of its absorption-curve in the spectrum; it follows therefore that to scale a wedge for extinguishing white light the absorption of E should be measured. The absorption by the brightest part of the spectrum, or by the whole of the rays, will not necessarily give the true extinction-value of the wedge. If a really black wedge were used, as all rays would be equally reduced, the measurement of total light, or that of any ray transmitted through different thicknesses of the wedge, would give the coefficient of the wedge for extinction purposes; but if a purplish or orange wedge were used it might not do so, supposing the absorption of E light was different from that of the other rays.

With a greenish wedge, such as is, I believe, used by Prof. Pritchard, the place of maximum luminosity of the spectrum seen through it would not differ much from E. I have a wedge myself in which the coefficient of absorption of the total light differs very largely from the coefficient of absorption of the E light. Such a wedge, if graduated in the ordinary way, would give erroneous results. Another point to be noticed is that star magnitudes obtained by extinction should agree better with those obtained by photography than those obtained by eye estimation; the first would be obtained by estimation of the E light, the second of the light between G and F, and the last of the light near D.

A red star would be most frequently of less magnitude to the eye than to the photographic plate, and by extinction would lie between the two. A blue star would be just the reverse, but the extinction magnitude would lie between the two.

There are two other points of interest in connexion with colour extinction. The first colour to go is red, and the violet or blue sensation goes last; it follows therefore that at twilight blue flowers can be seen for some time after the red flowers have become invisible, and insects are able to find the blue flowers to a much later time. The blue flowers get the best chance therefore of fertilization, and this may account for the large number of blue flowers compared to red. The second is with regard to the colours of faint stars: the Astronomer Royal has told me that Admiral Smyth and others find that stars of high magnitude (faint stars) are almost invariably blue; I can only say that if that be the case the facts I have put before you on this diagram show the reason of it. It does not follow that because these stars are seen blue or grey-blue they are not red; but their light is so faint that all colours appear grey, and, as the violet sensation disappears last, it frequently happens that you get the red and green sensations as grey, and the violet just above the colour limit, thus giving a grey-blue. If telescopes of larger aperture are used it might be possible to see these stars in colours; but I doubt very much whether these stars will be seen in colour in any other way.

The curve of distinct colour perception for the spectrum was shown by projection with the lantern. Unless the intensity of light of any colour is above this it appears as grey; hence, in very faint spectra, light is seen without colour being noticed, the whole of the light appearing grey.

The Astronomer Royal. When I made this remark to Capt. Abney I did not wish to be quoted as an authority on the subject of very faint stars. I was only giving my general impression. I think Admiral Smyth and Sir John Herschel have remarked that the very faint companions of stars are usually blue; but I should not be prepared to say anything beyond that. I am, however, quite inclined to accept Capt. Abney's explanation, though I should not like to pose as an authority on the colour of very faint stars. There is one point I would like to ask Capt. Abney, and that is as regards a very interesting remark he made about the accordance between the photographic magnitudes and those determined by the wedge photometer; whether, in the case of a star which is photographically bright, that is especially rich in the blue rays, it would be likely that a wedge would give the same magnitude as the photographs. He implies that the wedge would give a result intermediate between the visual magnitude and the photographic; but would the wedge agree more nearly with the photograph than with the eye estimation. I have been struck with the accordance between the photographic magnitudes and the photometric magnitudes determined at Oxford by means of the wedge in Prof. Pritchard's selected fields. In most cases the agreement is much closer with the wedge photometer in the cases I have examined than it is with the Argelander magnitudes. The point is whether there would be likely to be approximately a coincidence between the determination with the wedge of such stars and the determination from the photograph.

Mr. Plummer. I wish that Prof. Pritchard could have listened to the very interesting paper which has been read by Capt. Abney, because I believe he would have added something of great value to this discussion. I do not propose to enter into details with regard to the remarks of Capt. Abney, which appear to me as much physiological as they are photometrical. But I notice one circumstance which it seems might favour accurate observation with the wedge. So far as I understand the efforts of opticians to produce an achromatic object glass, the attempt is to combine rays of different refrangibilities in that portion of the spectrum which has the greatest luminosity; that is to say, rays about C and F are combined with rays about E, and therefore as extinction proceeds, in a refractor at least, the rays about E would be the last that would be extinguished; and this is of especial importance in a photometer used differentially, since the final extinction takes place on practically monochromatic light. I think this, to some extent, explains the point that the Astronomer Royal has raised, viz., the similarity of results arrived at from a photographic determination of magnitude and that derived from the wedge.

There is one matter I might point out to Capt. Abney, and that is that the action of the wedge on colour, as well as on photographic phenomena, was discussed with some completeness by Prof. Langley with the aid of the bolometer, and he showed that while the wedge permitted the transmission of 5 per cent. of the radiant energy measurable by that instrument, yet the transmitted portion of that energy perceptible as light by the eye was only one-tenth of 1 per cent.; yet such variation as this has not prevented the wedge from doing very good work. And that is the point with which I am more particularly concerned, the trustworthiness of the wedge as a photometer, and as a practical means for determining star magnitudes. The wedge determinations represent, on the whole, Argelander's magnitudes down to the ninth; and it is inconceivable that an instrument carrying on a regular curve should suddenly break down and be incapable of carrying on the same scale through one or two magnitudes. The present Nova affords a very good opportunity of testing the accuracy of wedge-observations. I have determined the magnitude of that star down to the twelfth magnitude, and if these observations agree with the magnitude determined by other means, it shows that the coefficient of absorption, however determined and for whatever light, is accurate for that wedge.

Dr. Spitta. What does Capt. Abney consider the best method of obtaining the coefficient of the wedge? That is to me the most interesting and important point; indeed it is the whole point of the amicable discussion between Prof. Pritchard and myself. Is it by extinction by the wedge or by prismatic extinction?

Dr. Common. I should like to ask Capt. Abney how his standard Amyl lamp differs from Harcourt's Pentane lamp?

Mr. Horner. There is one point raised in Capt. Abney's paper on which I should like to hear the opinion of Mr. John Brett. I refer to the statement that the majority of flowers are blue.

Mr. Brett. I wanted to make a remark, but I was afraid there were so many scientific gentlemen present that an artistic opinion would not be esteemed. Capt. Abney has used the term "normal vision," and I should like to know how he works out normal vision; because, if we do not know what normal vision is, it is no use discussing blindness of any sort. If he has any standard of normal vision I am sure all painters present would be glad to know what it is. Then he ignores the physiological aspect of the subject. It is known amongst painters that if you look at a red object for a short time it will cease to be red, and if you look at it for a longer time it will not be seen at all. Take a scarlet geranium—when you have worked at it for a quarter of an hour you will not know it is scarlet at all. You will go on painting it as if it were black or colourless.

Mr. Horner. How about other colours?'

Mr. Brett. Red is a very irritant colour to the retina; at least to the idiotic painter. (Laughter.) You cannot possibly see the red after having looked at it for half an hour. Whether this is normal vision I do not know. I will ask Capt. Abney to say whether it is or not. I have studied this for 40 or 50 years, and I have come to the conclusion that red is the most irritant colour in the spectrum to the retina.

A Member. Does that not show that it is easily perceived?

Mr. Brett. It is not perceived after a short time. You must consider that the eye is fatigued by looking at red very much sooner than by looking at blue.

Mr. Ranyard. With regard to what Capt. Abney has said as to the colours of very faint stars it is interesting to note that with the variable stars, which go through a range of many magnitudes, it has very frequently been remarked that they are much redder at the minimum than at the maximum. This may not be due to a physiological cause, but may be due to changes in the spectrum, for the violet end of the spectrum may vary more than the red end. I am inclined to think that the colour of small stars very much depends upon the correction of the object-glass you are using; and if the outstanding colour left by one optician is red, the extinction with the wedge will be different to the extinction when blue is left outstanding.

Mr. Petrie. I should like to ask a question with regard to the blue flowers. I have studied Botany for many years, and have found the greatest difficulty in getting good blue flowers; they are very scarce. It is a well-known fact in elementary Botany that blue flowers are difficult to keep in a garden, and if this is so for a garden it will be so amongst wild flowers.

Mr. Teasdale. The predominance of colour depends upon the weather. In the spring it is white, and it passes on to the red end of the spectrum towards the autumn.

Capt. Abney. The Astronomer Royal has asked whether the magnitudes of blue stars ought to be more nearly coincident as determined photographically and by extinction than they are if determined photographically and visually in the ordinary way. That undoubtedly is the case. The wedge would agree much more nearly with the photographic results than eye estimations would. Eye observations take into account the red and green sensations as well as the violet, but the photographic plate and the wedge measure the violet light and the violet sensation respectively, hence they are nearer together than the eye and photographic plate.

In reply to Mr. Plummer, I would point out it is the wedge which extinguishes and not the object-glass; that is to say, although the optician may make G and C coincide, the thickness of wedge necessary to extinguish E is nothing to do with the refractor whatever, except as to size of the disk. I will not pin my faith upon that, but at the first blush I should say it has nothing to do with the size of the disk. In order to get the extinction you have to extinguish the E light. It is the wedge that extinguishes and not the refractor.

Mr. Plummer. As a comparative and differential instrument, I take it your observations are made on the E line.

Capt. Abney. Not my observations; yours are. My observations are made over the whole spectrum, but yours are confined to the E line. If you take observations of different thicknesses, you get the scale of the wedge and in no other way. When you are extinguishing with the wedge you will find it excessively difficult to say when you have extinguished the red or green light. Captain Abney explained this by means of a lantern-slide.

Dr. Spitta. The grey light would go out before the violet?

Capt. Abney. Long before; and the grey light of the red will go out long before the colour of the green. The light in the red part of the spectrum would disappear long before the green light would. I think Mr. Plummer will see that is an answer to his question. Now Dr. Common asked what is the difference between the Amyl lamp and the Pentane lamp. They are different and they are not different. The Amyl lamp is a very convenient one and the Pentane lamp is a very inconvenient one. The former lamp uses a liquid which passes through a wick and evaporates, whereas in the latter you have to use a jet and force gas over a large surface of liquid to evaporate it before burning.

Dr. Common. It is as to the colour that I wish to know.

Capt. Abney. The Amyl lamp has more colour than the Pentane; I, however, prefer to use the first-named lamp. Mr. Plummer referred to Mr. Langley; I say he has not measured by the extinction of light at all. He has measured the acuteness of vision in different parts of the spectrum, which is a very different thing to the extinction of light. I am quite aware of Mr. Langley's researches, but, interesting as they are, they do not quite agree with some other results. Now Mr. Brett is always interesting at all events. He has always the happy knack of making most apt remarks, and I think on this occasion he has been normally successful. He asked me to tell him what is normal vision. Well, I do not know that Mr. Brett will understand if I tell him. [Laughter.] There are only 5 per cent. of mankind who do not possess normal vision. The large majority can measure the spectrum in the same way that I can. I stand here as a specimen of normal vision, and there are 95 per cent. who see the spectrum and colours in the same way as I do. Now Mr. Brett is extra normal [laughter], that is to say, that he perceives colours that I should pass over; but that is a matter of education. 99·6 per cent. of females also have normal vision. Mr. Brett spoke about the physiological aspect, and said if you look at a red object for a quarter of an hour you will not see that it is red at all, but that it is black. I want no further confirmation of what I have said than Mr. Brett's remarks, because the red sensation is very fatiguing, and what I think is popularly called the red nerve is easily fatigued. If a quarter of an hour makes anything black, it only shows in that time the red nerves have become useless. I am glad Mr. Brett brought up the red, because this is absolutely confirmatory of what I said.

Mr. Brett. You can look at the green until all is blue, Sir. [Laughter.]

Capt. Abney. I think that remark of Mr. Brett's is absolutely confirmatory of what I said. With regard to the colours of faint stars: if you put a poker in the fire and make it red hot, and then carry it into a dark room, the last colour you will see is grey. In exactly the same way faint red stars appear grey. With regard to the colour of variable stars a very interesting point arises. They must be very rich in red, showing that they are, comparatively speaking, very cool indeed, because to appear red they must be 1500 times more rich in red rays than in violet. It may be a difference in the atmosphere which gives a difference in colour, but temperature is more likely the reason. I am not a horticulturist, and I know nothing about flowers. I made the statement about red and blue flowers on the authority of Prof. Huxley, and cannot give any results from my own observations. But the fact remains that the last colour to be seen in a field or in a garden is the blue or the violet, and that may be the reason why blue and violet flowers predominate.

Mr. Kennedy. I think what Capt. Abney says about flowers is well authenticated. There are only two red wild flowers—the pimpernel and the poppy, whereas there are hundreds of blue flowers.

The Chairman. There is one interesting result from this discussion: I see now why we dress our soldiers in red clothes.

A vote of thanks was accorded Captain Abney for his paper.

The Chairman. We have the honour of the presence among us to-night of Prof. Oudemans, one of our Foreign Associates, and I will ask him to favour us with a few remarks about the triangulations of Java on which he has recently been engaged.

Mr. Turner. Before Prof. Oudemans' address I should like to call attention to a paper on "The Reappearance of Saturn's Rings" by Mr. Barnard. Up to 1891 Oct. 29 Mr. Barnard was not able on any occasion, with the most careful scrutiny and good seeing, to detect the slightest trace of the ring. It was possible, however, after looking at the black line of the rings across the ball, to get the momentary impression of the presence of the rings against the black sky, but this, I am convinced, is an optical phenomenon similar to the "after image" produced on the dark part of a room by gazing at the window fixedly and then turning the eyes suddenly away. On Oct. 29, at $17^h\ 0^m$ Mount Hamilton Mean Time, the rings were easily and distinctly visible as slender threads of light. It was not possible, however, to see them at their junction with the ball, they being invisible for a space of about 2″ through nearness to the bright disk. Perhaps this was also due to the fact that the ansæ would be brighter at a distance equal to the inner edge of the bright rings. At the time of observation the Earth was 1° 58′ north of the plane of the rings, while the Sun was exactly in their plane according to the ephemeris. Prof. Oudemans says ('Monthly Notices,' Jan. 1892) on Oct. 29, $17^h\ 30^m$ (=Oct. 29, $9^h\ 3^m$ Mt. Hamilton M.T.) Saturn was decidedly without any ring. Oct. 30 was cloudy, and on Oct. 31, at $17^h\ 30^m$, a momentary view showed the ring at both sides of the planet as a thin bright line. Combining my observations with those of Prof. Oudemans for the same date, it is evident that the time of reappearance of the ring is narrowed down to the interval between $9^h\ 3^m$ and $17^h\ 0^m$, Oct. 29, 1891, Mt. Hamilton M.T. The American ephemeris gives Oct. 30 as the time of reappearance.

The Chairman then called upon Prof. Oudemans.

Prof. Oudemans, Foreign Associate, who spoke in English, gave an account of the triangulation of Java, executed principally under his direction in the years 1854 to 1880, the reduction of which is now nearly finished at Utrecht *. This triangulation was originally commenced to form a basis for a military and topographic map of the islands of Java and Madura; but the horizontal measures made with altazimuths by Pistor and Martins and by Repsold proved exact enough to attain a higher signification, and the Home Government, on a proposal of the Royal Academy of Science at Amsterdam, decided that the triangulation should also serve "for the measurement of degrees," which is to be understood as meaning, in the present state of Higher Geodesy, the study of the irregularities of the figure and the attraction of the Earth.

For the primary triangulation 10-inch altazimuths † by Pistor and Martins and a 12-inch altazimuth by Repsold were employed, with a few exceptions. Occasionally, also, 8-inch altazimuths by Pistor and Martins have served; but generally these have been employed for the secondary measures, as also have some 6- and 5-inch instruments. These latter were read by noniuses, but the 8-, 10-, and 12-inch instruments by microscopes.

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* The Royal Astronomical Society has already received three parts of the Report of this work, the first part containing the description of the base-line apparatus (constructed by Repsold) and of the comparitor joined to it, and the comparisons of the rods with the "normal meter"; the second part containing a report on the measuring of a base-line near Buitenzorg in West Java; and the third part containing reports on the measurings of base-lines in Middle and in East Java, and the comparison of the normal meter with the platinum iridium method at 27°.

† The inches here meant are Paris inches (1 Paris inch = 1·066 English inches).

In each of the instruments of Pistor and Martins the telescope was on the end of the horizontal axis, in that of Repsold in the middle; but there it was broken, *i. e.*, by means of a rectangular prism in the cube the rays were reflected horizontally to one of the ends of the horizontal axis.

Generally the original plan was that the primary angles should be found by 6 pairs of measures, the telescope being in each pair first to the left and then to the right. After every pair the horizontal circle was turned by 30 degrees; so, as there were two microscopes read, the angles were measured along twelve different arcs of the limb.

In the Primary Triangulation signals were avoided; but heliotropes were stuck in holes bored in the axes of the pillars, so the reduction to the centre was unnecessary. In the Secondary Triangulation bamboo signals, in the form of a double hexagonal pyramid, were used. When possible this pyramid had its axis coincident with that of the pillar, although sometimes signal and pillar were separated from each other, and then the reduction to the pillar was necessary.

The network of the two islands Java and Madura (of which a sketch was given on the black-board) contains 125 primary stations. The ends of the base-lines and the stations of the nets connecting these base-lines with the primary triangles make 20 stations more, so that in all there were 145 primary stations.

With the apparatus made by Repsold, three base-lines were measured, one in West, one in Middle, and one in East Java. Each measure was done twice, and by dividing the base-lines into divisions of 200 metres each, and by measuring each of these divisions twice, it was possible to ascertain the mean error of every 200 metres measured, and accordingly of the result for the whole base-line, with very great accuracy.

The details of the bases measured are:—

	West Java.	Middle Java.	East Java.
Length of base-line..	3900^{m}	4175^{m}	5041^{m}
Mean error	$\pm 2^{mm}\cdot 35$	$\pm 0^{mm}\cdot 464$	$\pm 0^{mm}\cdot 609$
Probable error......	$\pm 1\ \cdot 58$	$\pm 0\ \cdot 313$	$\pm 0\ \cdot 411$
Fraction	$\frac{1}{2,500,000}$	$\frac{1}{13,331,000}$	$\frac{1}{12,265,000}$

In the nets connecting these base-lines with the primary network the corrections of the observed directions have been calculated by the method of least squares, but the amount of work was such that the possibility was considered of avoiding it for the whole of the triangles. To reduce the whole of the triangles by least squares would have meant an additional work of perhaps two or three years, and what sort of work! A work, which General

Perrier once named "un travail de Bénédictin," the Benedictine monks having been notorious at all times for the possession of patience for any useful work, however long and monotonous.

Mr. M. L. J. van Asperen, pensioned Colonel of the Dutch Navy, who assists me (Professor Oudemans) in the calculations of the triangulation, tried to correct the observed directions on a simpler way, such as used by surveyors (the oblong shape of the island being favourable to applying this method), not disdaining, however, the method of least squares, when there was a quadrangle on a pentagon, and its diagonals observed as well as the sides. At first this solution was not intended to be used for a definitive one, the chief purpose being to try how the three bases would correspond, but happily the event was so strikingly advantageous, that the necessity of a more rigorous solution was judged to be in no ways indicated.

In fact, to make the three base-lines correspond, through the already adjusted network of triangles, the following small corrections were necessary in their logarithms:—

Base-line of West Java +0·0000009,
" Middle " −0·0000013,
" East " +0·0000004.

Everyone will admit these corrections to be small enough to be applied without risk of serious error, and so the adjustment which was performed within three months was retained.

By the spherical excesses of the triangles alone the mean error of every concluded direction or bearing was deduced $= \pm 0''{\cdot}81$. It could be expected that the corrections found by the adjustment just mentioned should be larger, and in fact it was found $\pm 1''{\cdot}09$, only one third more than by the triangles.

In order to render the triangulation of Java tributary to the knowledge of the figure of the Earth, a large number of latitudes and azimuths have been determined. At first I considered, in consequence of the extension of the island of Java in the east and west direction, that seven pairs of stations would be sufficient for the determination of latitude, every pair consisting of a station near the north coast and of a corresponding station near the south coast. But as Mr. Metzger, one of the engineers, once found an indication of a very strong local attraction (of about 40″) at the south coast, the number of stations was doubled, the number of observations to be taken on either station being, however, reduced to one half.

At every station where latitude was to be determined, an azimuth was also to be taken, and the southern polar star being too low and too feeble to be easily found, Kaiser's idea was followed to use stars near the equator at a height of 10° to 30°. The azimuths of these stars, which were always observed to the east and the west, change very slowly, and are thus very suitable to replace the southern pole star. The horizontal distance between such a star and either a distant signal of the triangulation or a night-signal, put up at a distance of a few hundred metres, was then measured, using the same programme as for the primary angles.

Unhappily, the new series of latitude observations has not fully been carried out. I was called home again, by the death of Prof. Hoek, to take his place at the University at Utrecht, and left Java in 1875. While I was in Java a large portion of the staff of observers had either left the service with pensions or died, and since 1875 many more have left from similar causes.

As soon as the lengths of all the sides of the triangles were calculated, the latitudes and longitudes of all the stations, secondary as well as primary, were to be deduced. At Genook, the northernmost primary station in the Residency Japara, an excellent latitude and azimuth had been determined, and this station was taken as the starting-point. Adopting the constants for the Earth spheroid of Bessel, the latitudes and longitudes of all the other stations, as well as the azimuths, were deduced. The longitude of the time-signal at the old harbour of Batavia was thereby made $=0^\circ\ 0'\ 0''\cdot00$. The differences between the calculated and the astronomical latitudes were sometimes positive, sometimes negative, so that it seemed not necessary to alter the initial latitude of Genook. The same was the case with the azimuths.

Differences of longitude by telegraph have not yet been determined with the utmost accuracy, though this sort of operations was also on the programme, but as far as they have been made for geographical purposes (*vid. f. i.* 'Astron. Nachrichten,' Bd. 74, pp. 164, 165) the telegraphic differences of longitude correspond very nearly with the geodetic ones.

The calculations finished, a research is now being carried on on the mean errors of reading the circles, of the divisions of the circles, of the pointing of the telescopes. The results will be given in the Report. I would, in conclusion, like to mention the remarkable constancy found in the mean error in pointing and reading for the same observer, using the same instrument, but at different places.

A vote of thanks was passed to Prof. Oudemans for his address.

The Chairman. I am instructed by the Council to say that the question of the election of women having come before them, it was resolved not to take any action in the matter as to their admission, but to leave it to the Fellows to take their own course. By that statement it must appear that the Council were not prepared to say that their admission was illegal, and therefore the first election is left in the hands of the Fellows present. At the same time any Fellows who give a vote must clearly understand that it is a question whether it is advisable that they should be admitted to the Society or not, and they must each decide for themselves if it is for the benefit of the Society as a whole whether they should be elected or not. With these remarks I will simply leave it in the hands of the Fellows.

Mr. Brett. The President has left it in the hands of the Society, that is, in your hands and mine. This is a very interesting subject, but he calls it a serious one. I am afraid we shall find it a serious one, because it is practically a proposal to introduce into these dull meetings the social element, and all we shall require is a piano and a fiddle, and then we shall be able to get a few pounds whereby we shall be able to lay down a parquet flooring, and I am sure many of my young friends will be glad to dance through most of the papers.

Capt. Abney entered a protest against the legality of the election in case the women should be elected.

The following papers were announced:—

Arthur Mee. "Note on the Transit of Titan, 1892, March 11."

S. W. Burnham. "The Double Star Σ 3123."

S. W. Burnham. "The New Star in Auriga."

E. E. Barnard. "Observations of the Reappearance of the Rings of Saturn; Observations of the Position-angles of the Rings and Observations of the Satellites."

George Knott. "The New Star in Auriga."

Ernest W. Brown. "Note on the Lunar Theory."

A. Marth. "Ephemeris for Physical Observations of Jupiter, 1892."

Radcliffe Observatory, Oxford. "Estimations of Magnitude of Nova Aurigæ." Communicated by Mr. Stone.

Prof. K. D. Naegamvala. "Note on the Spectrum of the Great Spot Group of February 1892."

Capt. W. de W. Abney. "On the Estimation of Star Magnitudes by Extinction with the Wedge."

Louis Joyner, Bermuda; *Belgrave Ninnis, M.D., R.N., F.R.G.S.*, 46 Kensington Park Road, W.; *John Krom Rees, A.M., E.M.*, Director of the Observatory and Professor of Practical Astronomy and Geodesy, Columbia College, New York City, U.S.A.; *William James Watson*, Morley House, South Stockton-on-Tees, were duly elected Fellows of the Society.

The three lady Candidates were not elected. It will be remembered that by the Bye-laws of the Society a Candidate must have three-fourths of the votes in his favour in order to be elected.

The following Candidates were proposed for election as Fellows of the Society:—

Rev. James Baikie, Wood Inch, Dalguise, near Dunkeld (proposed by W. S. Franks); *John Dansken*, 4 Eldon Terrace, Partick, Glasgow (proposed by Prof. R. Grant); *H. D. McCarthy*, Cheltenham (proposed by J. McCarthy); *Martin Charles Sharp, M.A.*, 5 Portman Street, Portman Square, W. (proposed by Rev. T. E. Espin).

BRITISH ASTRONOMICAL ASSOCIATION.

The fifth ordinary Meeting of the second session was held in Barnard's Inn Hall on Wednesday, March 30, Capt. Noble, the President, in the chair. Nine new members were elected, and a list of fifteen candidates for election was read.

Mr. Duke called attention to the fact that six of the fifteen candidates had been introduced by Mr. Schooling.

Several presents had been received during the month, among which might be mentioned "Comparison Photographs of High Sun and Low Sun Spectra," by Mr. F. McLean, F.R.A.S., and several works of Sig. Bianco, presented by the author.

Miss Everett read a paper on the "Photographic Magnitude of Nova Aurigæ," and illustrated it by a diagram representing the light curve obtained from measurements of the photographs taken at the Royal Observatory, Greenwich, with the 13-in. equatorial mounted for the International Survey. Miss Everett described the way in which the photographs were measured and the method of deducing the magnitude from the diameter of the image.

Mr. Maunder read part of two papers on the same subject, one by Mr. J. E. Gore and the other by Mr. Jos. Baxendell. He pointed out that the results of these two observers corresponded almost exactly, and that the great point of difference between their light curve and Miss Everett's photographic one was that while the latter showed a very marked maximum on the 18th, it was only very feebly represented in the former.

It had occurred to him that the appearances of the spectrum reported by Prof. Vogel might be due to a single star with a large swarm of meteorites coming very nearly to periastron in the line of sight, and that this hypothesis would also explain the rapid fall in brightness of the star.

Mr. Holmes then read a paper entitled "A few Notes upon Reflecting Telescopes." He described a simple method of testing the correction of a telescope and a mounting for mirrors up to 13 inches. He said that air currents in the tube due to wind could be prevented by placing a screen equal in height to the diameter of the tube upon the windward side. Concerning the silvering of mirrors he remarked that we were greatly in want of a good and reliable process.

Mr. Seabrook had found that it was better to mix the solutions a day or two before using them.

The Rev. A. Freeman then read two papers upon the "Dark Transit of Titan and his Shadow on Mar. 11th" and "Saturn's Rings and Belts on Mar. 12th." Mr. Freeman had observed this transit under very favourable circumstances, and had also received a postcard from Mr. J. W. Ward, of Belfast, with an account of his observations of the same transit. He had observed a pale red equatorial band rather less dark than the crape ring, and invaded in three long elliptical patches by the N. bright equatorial belt. This had since been confirmed by Mr. A. Stanley Williams. His observations tended to show that if there were an atmosphere to the rings it would probably only lie over the crape ring and possibly a little over the bright ring.

Whilst the ballot was proceeding Mr. Maunder read a paper by Mr. Wooster upon the "Conjunction of Jupiter and Venus."

In the Special General Meeting held after the ordinary meeting it was decided that, in the case of any Society wishing to join the Association as a Branch, the members of that Society should be considered original members of the Association and the payment of entrance fee should be optional.

ROYAL METEOROLOGICAL SOCIETY.

The usual monthly Meeting of this Society was held on Wednesday evening, the 20th April, at the Institution of Civil Engineers, 25 Great George Street, Westminster, Dr. C. Theodore Williams, President, in the chair.

Reference was made to the death of Dr. J. W. Tripe, who had held the office of Council Secretary for the last 20 years, and a resolution of sympathy with the family was passed by the Meeting.

Señor R. Aquilar y Sautillan, Sir Andrew Clark, Bart., F.R.S., Mr. F. W. Cross, Assoc.M.Inst.C.E., Mr. H. Hancock, M.A., Mr. W. B. Haberden, Dr. Hermann Weber, and Mr. E. R. Williams were elected Fellows of the Society.

The following papers were read:—

"Anemometer Comparisons," by Mr. W. H. Dines, B.A., F.R.Met.Soc. This was a report on a valuable series of experiments which have been carried out at the request of the Council of the Society, with the view of obtaining a direct comparison of the various anemometers in common use, so that some opinion might be formed as to which type of instrument is the most suitable for general purposes. The Meteorological Council have defrayed the cost of the work. The anemometers which were compared were:—1. Kew Pattern Robinson; 2. Self-adjusting Helicoid; 3. Air Meter; 4. Circular Pressure Plate (one foot in diameter); and 5. A special modification of Tube Anemometer. Most of these instruments are of the author's own invention, as well as the apparatus for obtaining automatic and simultaneous records from all the instruments upon the same sheet of paper. It appears that the factor of the Kew Pattern Robinson is practically constant and must lie between 2·00 and 2·20. The Helicoid Anemometer is quite independent of friction for all excepting light winds, and different sizes read alike; but it is not so simple in construction as the cup form. The Air Meter consists of a single screw-blade formed of thin aluminium, and made as nearly as possible into the exact shape of a portion of a Helicoid. A similar instrument, with a larger blade and with the dial protected from the weather, would probably form a useful and correct Anemometer. It would be light and offer a very trifling resistance to the wind. The oscillations of the pressure-plate must have been considerably damped by the action of the floating weight, but, as it was, they were sufficiently violent. It seems probable that the remarkably high values sometimes given by the Osler Pressure Plate may be due to the inertia of the moving parts. The Tube Anemometer appears to possess numerous advantages. The head is simple in construction, and so strong that it is practically indestructible by the most violent hurricane. The recording apparatus can be placed at any reasonable distance from the head, and the connecting-pipes may go round several sharp corners without harm. The power is conveyed from the head without loss by friction, and hence the instrument may be made sensitive to very low velocities, without impairing its ability to resist the most severe gale.

"The Hurricane over the West Indies, August 18–27, 1891," by Mr. F. Watts. The author has collected a number of observations on this violent hurricane, which on August 18 swept from the Atlantic into the Caribbean Sea and moved in a north-northwesterly direction over San Domingo, and thence northward and eastward. At Martinique the barometer, which at 5.30 P.M. stood at 29·80 in., fell to 28·38 in. at 8.15 P.M. during the passing of the centre of the cyclone.

Selenographical Notes.

BULLIALDUS.—This magnificent object is an instructive example of that type of ring-plain which though distinguished by a deeply sunken concave floor, with a complex central mountain, and by terraced walls (to a great extent undisturbed by subsequent formations) surrounded by buttresses and long radiating spurs, yet differs from the majority of its class in exhibiting no indications of light streaks or ray phenomena. These far-extending spurs are more largely developed in connection with Bullialdus than in the case of any other lunar ring of a similar description, Copernicus, Aristoteles, and Aristillus alone excepted; but they are only seen to advantage under such favourable conditions of phase and definition as only occur at rare intervals.

The most striking features at sunrise in connection with the circumvallation are the two concentric valleys, with the intervening terrace, about halfway down the bright inner slope of the east wall,—extending from an oval depression on the north-east to a shallow break in its continuity on the south-east, caused apparently by the presence of two or more small craters, one of which is easily glimpsed under a morning Sun with a moderately high power. On the western wall Schmidt shows several craters with rims, but, as they are only traceable for a short time, they appear to be merely depressions of no great depth. The largest is on the south-west, near the end of a narrow valley running from the east side of Bullialdus A to the summit of the wall of Bullialdus.

If viewed when the morning terminator ranges from 25° to 27° east long., three or four of the little valleys between the radiating spurs from the south-east wall have all the appearance of clefts, especially that running up to the north side of Bullialdus B, which has often been so recorded with a low power.

Of the three fine ring-plains associated with the formation on the south, C is the largest and the deepest, and, though it is not, I believe, elsewhere noticed, is surrounded by a silvery lustre which recalls the large craters south-east of Landsberg. This is especially remarkable on the south-western flank of C, where it ultimately narrows into a bright streak which traverses Kies, and extends some distance to the south of this formation. The little craterlet on the north wall of C is an easy object, though omitted by Schmidt. Neison shows another on the opposite rim, which I have not seen. The prominent shadow of C under a low morning Sun exhibits a long spire of shade, which appears to represent a lofty peak on the wall of the ring-plain, though it is really due to a rocky wall pertaining to an incomplete formation on the north-east side of it. Schröter remarked and drew this peculiar shadow (see Sel. Top. Frag. t. li. fig. 5). Beyond its great depth and its clearly terraced wall, there is nothing remarkable about Bullialdus B, though there is a fine dark line connecting it with the foot of the east border of Bullialdus A, which, at sunrise, greatly resembles a cleft. It is hardly a valley between the radiating spurs, as it strikes across them nearly at right angles. Schmidt and Neison draw A, B, and C as circular. A has certainly a polygonal outline and its floor is bounded by five sides,—the regularity of one of the western sections being broken by a small crater. There is a crater row beyond the wall on this side, which is well shown by Schmidt, running up to, and partly through, the south border of Bullialdus, and a crater on the south wall, which I have only seen once. Schmidt's Monograph of Bullialdus and the surrounding region, reproduced in Proctor's "Moon," though on a somewhat smaller scale than his chart, shows more detail in connection with A, B, and C, and differs considerably from the latter in the representation of the central mountain and floor of the great ring-plain.

Some twenty years ago, Mr. H. Michell Whitley, of Penarth, Truro, drew attention, in the 'Astronomical Register,' to groups of crater-chains on the south-west of Bullialdus, stretching away from near the hill marked γ on Beer and Mädler's map, and crossed at a short distance from γ by another chain of a similar character. Though the individual craters are described as being smaller than those which compose the well-known chain north-west of Copernicus, their appearance is said to be almost as remarkable. Neither Schmidt nor Neison show anything answering to these objects, which are probably very evanescent, and only to be seen at a particular phase under the best conditions. The former draws a crater-row a short distance south of Bullialdus D, trending from east to west; but this does not agree with the position of Mr. Whitley's transverse row, which must lie more to the north.

Neison refers to a peculiarity in connection with this crater D, which shows the importance of viewing an object, even though it be of considerable size, under more than one condition of illumination before deciding as to its true character. From the fact that one of its walls is much higher and steeper than the other, it is only visible as a mountain under a morning Sun, while in the waning Moon it is seen to be a very distinct crater.

There is a curious triangular valley enclosed by broad low ridges lying between Lubiniezky and the east wall of Bullialdus, which is a very remarkable object when near the terminator. On May 4, 1884, 8^h 40^m, I noted a short delicate cleft crossing a strip of high ground north of it, which is not shown in the maps.

Beaumont House, Shakespeare Road,
Bedford, 1892, April 19.

THOS. GWYN ELGER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Lunar Crater Fra Mauro B.

GENTLEMEN,—

Will you allow me a few lines to correct an error into which Mr. Gwyn Elger has fallen in your March number regarding the lunar crater Fra Mauro B? In a letter addressed to him I stated that, according to an observation of my own made with a 2-inch spy-glass and power of 64, Neison in his Map XIII. misplaces this crater *with relation to* the two formations Fra Mauro A and Σ; further that B. and M. map these formations in their true *relative* positions, and in these statements I was perfectly correct. If Mr. Gwyn Elger finds that Neison puts Fra Mauro B in the correct latitude and longitude, this only proves that Neison misplaces one or both of the other two formations, but does not invalidate my observation in the least. Neison puts all these formations in very nearly the same straight line, and is wrong in so doing.

Mr. Gwyn Elger expresses surprise that I saw the little crater *f* south of Gambart. This crater was easy in comparison with Fra Mauro B, which last I consider perhaps the most difficult object I ever saw, in spite of the fact that with a 6-inch telescope and power of 125 I once independently picked up the "granular texture" of the principal condensation of the Andromeda nebula.

Yours truly,

Berkeley, Cal., 1892, Mar. 25. Roger Sprague.

Transit of Mercury in 1278.

Gentlemen,—

When looking over the Chronicle of Conrad Lycosthenes I found a notice of a transit of Mercury in the year 1278. After alluding to, what I suppose he means, an eclipse of the Sun, he says:—"Mercurii stella visa est in medio sole velut macula nigra." I suppose this to be the *earliest* mention of this phenomenon on record.

Yours faithfully,

The Observatory, Crowborough,
Sussex, 1892, Apr. 12. C. Leeson Prince.

On some Early Comets.

Gentlemen,—

In 'A General Chronological History of the Air, Meteors, &c.,' 1749, by an anonymous author, there is a comet recorded in the year 1171 that seems to have exhibited a similar freak to that of Biela's in 1845. The account runs as follows: "At Christmas were two stars of a fiery colour seen in the west: one was great, the other small, they seemed joined at first, then separated and a great way distant." The fact of a comet having been witnessed in 1099 and of a display of meteors in 1243 might seem a further resemblance, only the meteoric display seems an early return of the Perseids. The above comet does not seem to be recorded by Williams among those seen in China or yet mentioned in Chambers's 'Catalogue.'

Other comets mentioned in the above-named work alone are the following, though of course it is possible that some of them may be recorded under different dates elsewhere:—

A.D. 687. After mentioning a new star, the writer says: "In February after, a comet rose out of the west, and with great brightness went to the east."

1059. "A comet shone in Poland for some days." Chr. Magdeb, Func.

1077. "April the 16th being very fair and clear, appeared a comet or blazing star."

1103. "On the first Quadragesima week, about even, the first day, an uncommon star was seen in the west for 25 days together, and shined at the same hour."

1219. "In England appeared a stupendous great comet." Chr. Magdeb.

Further search reveals comets in the first half of the seventeenth century, that seemed so barren of them, Arago recording only two in that period. In the work alluded to we have mention made of "a comet in November," 1612, and in 1633 we are told that "a comet like a Launce hung over Barcelona."

Melplash Vicarage, Dorset,
1892, April 14.

Faithfully yours,
S. J. JOHNSON.

Apparent Variation in the Brightness of Stars.

GENTLEMEN,—

In his interesting paper read at the last meeting of the Royal Astronomical Society,—a paper both interesting in itself and in the lively discussion to which it gave rise,—Captain Abney told us that, in the extinction of the colours of the spectrum, the red goes first and the blue is the last to survive.

This may have some relation to a phenomenon noticed by myself, and I believe by some other observers also, that if a white and a red star in the same field of view appear, with a given telescopic aperture, to be equal in magnitude, then as the aperture is decreased the red star degrades more rapidly than the white one. To put it in another way,—I have noticed that if two stars, one red and the other white, appear to be nearly equal in brightness with my 7⅓-inch refractor, and the eye is transferred to the 2-inch finder, the red star will appear some tenths, perhaps half a magnitude, fainter than its white neighbour.

This phenomenon, due to physiological causes I presume, gives rise to a difficulty in coordinating observations of very red variable stars made by different observers. Although ninety-five per cent. of us may have "normal vision," it is likely that the physiological personal equation in this case may be different for different observers, and perhaps also for the same observer at different times.

Knowles Lodge, Cuckfield,
1892, April 13.

Yours faithfully,
GEORGE KNOTT.

Diameter of Titan's Disc.

GENTLEMEN,—

Dark transits of Titan have now been observed, March 11th by myself, Mr. A. Mee of Llanelly, and Dr. F. Terby of Louvain; March 27th by Mr. J. W. Ward of Belfast; April 12th by Mr. A. S. Williams of Burgess Hill. On the last occasion Titan was seen as a dark object travelling along the chief south dark belt. It is evident therefore that the albedo of Titan is decidedly inferior to the average albedo of Saturn, and, consequently, that photometric measures of the diameter of Titan which assume the equality of its albedo with that of Saturn must give results for the magnitude of Titan's disc decidedly too small. It is a matter of particular interest that the late Rev. W. R. Dawes, in his description of a transit of Titan's shadow observed by him on April 15th 1862, and described in 'Monthly Notices' of Royal Astronomical Society, Vol. xxii. pp. 264–6, anticipates dark transits for the following reasons:—"The size of the shadow is, however, far greater than I was prepared to expect. I carefully estimated it at little less than 1″ in diameter—certainly not less than 0″·8. It may reasonably be concluded from this that some part of the surface of the satellite is not very reflective; and that with a large amount of optical power it might perhaps be perceived as a *dark spot* on the disk of the planet (as is *always* the case with the third and fourth satellites of Jupiter), especially if it should happen in its transit to be projected on the bright equatorial region." The diameter of Titan known to Dawes was 0″·75 as estimated by Schroeter, Mädler, and W. Struve. Mr. Ward saw the dark Titan with a 4·3 inch Wray refractor, power 113, slightly south of the chief dark belt. I saw it with a 6½-inch refractor a little south of the second south dark belt. In both cases Titan was seen dark on regions less bright than the equatorial belt. It is even darker, according to Mr. Williams, than the chief south dark belt. My estimates of magnitude, made by comparison with the breadth of the ansæ next the limbs, will give, at the mean distance of Saturn, 0″·976 for the diameter of the shadow, and 0″·650 for the diameter of the dark disc of Titan in transit, as I have recently stated to the British Astronomical Association, March 30th. The diameter of the bright disc of Titan at his grazing transit, January 21st, 1891, I did not estimate, but believe it to have been not much less than 0″·75.

Faithfully yours,
A. FREEMAN.

Murston Rectory, Sittingbourne,
1892, April 16.

Eclipses mentioned in the Anglo-Saxon Chronicle.

GENTLEMEN,—

There is no difficulty, excepting in a few cases, in identifying the eclipses mentioned in the Anglo-Saxon Chronicle. To each of these exceptions I will allude separately.

An eclipse of the Sun is said to have taken place in A.D. 807, on the 17th day before the calends of August, which would correspond to the 15th of July. As the Moon was in conjunction that year on the 9th of July and the 7th of August, no such eclipse could have taken place. Eginhard records an eclipse of the Sun which occurred in that year on the 11th of February, and two of the Moon on the 26th of February and the 21st of August. He also mentions an occultation of Jupiter on the 31st of January and a so-called passage of Mercury over the Sun's disk, which was said to have been observed during eight days in March and must really have been occasioned by a large and persistent spot on the Sun.

An eclipse of the Moon is recorded in the Chronicle on the fifth day before the calends of April in the year A.D. 795. Now as one occurred, not on that day (March 27) but on the 9th of April, it is only necessary to alter the record from the fifth day before the *calends* to the fifth day before the *ides* of April. An eclipse of the Moon is also mentioned in "midwinter's mass-night" (*i. e.* Christmas Eve) in A.D. 827; in this case, as one occurred on Dec. 24, A.D. 828, we have only to suppose that the historian made a mistake of one year in the record of the eclipse.

But the most interesting of these cases, in an historical point of view, is the eclipse of August 2, A.D. 1133, which was very large in this country and total in Scotland. The Anglo-Saxon Chronicle ranges it under A.D. 1135, the year in which Henry I. died; but overlooks that that king spent his last two years in Normandy, for it speaks of the eclipse as taking place whilst he was crossing the Channel for the last time, "the day after Lammas," which was in A.D. 1133. Mr. Johnson ('Eclipses Past and Future,' p. 47) refers to William of Malmesbury as speaking of the eclipse in connection with the king's death; but in this he is wrong, for the event which that historian states took place at the time of the eclipse (of which he was an eye-witness and says that he saw the stars appear) was not the king's death, but his "extremum transitum," or last passage to the Continent, which was more than two years before his death. He gives the same date in the year for the latter event as the Anglo-Saxon Chronicle, for he says that it occurred on the calends of December, and the Chronicle on "the next day after St. Andrew's mass-day." The eclipse therefore settles without any possibility of doubt the day on which Henry I. last left England. The late Prof. Freeman ('Norman Conquest,' vol. v. p. 239) accounts for the error in the Anglo-Saxon Chronicle by saying:—"The fact that he [Henry I.] never came back to England, together with the circumstances of his voyage, seems to have made a deep impression on men's minds. In popular belief the signs and wonders which marked his last voyage were transferred to the Lammas-tide next before his death two years later."

Yours faithfully,

Blackheath, 1892, Mar. 14. W. T. LYNN.

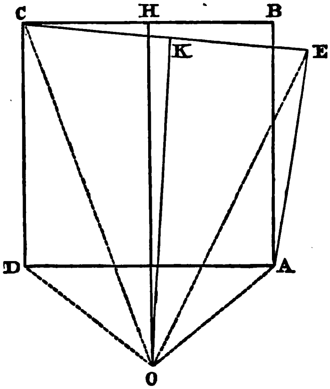
PUBLICATIONS.

AUSTRALIAN LONGITUDES.—A publication recently issued from the Hydrographic Office gives the results of longitude determinations by Commander W. U. Moore, R.N., Lieut. Parry, R.N., and Mr. C. Todd, Government Astronomer at Adelaide. There are also a number of chronometric meridian distances between places on the N.W. coast of Australia. In the telegraphic longitude work Comm. Moore and Lieut. Parry, at one end of the line, independently determined the chronometer error by sextant observations and observed Mr. Todd's signals. The total distance travelled by the signals was 3567 miles, and several repeaters had to be used. In the result we have definitive Longitudes and Latitudes for twelve places, amongst them being—

	Long. E.	Lat. S.
Brooma, Lookout Beacon	122° 14′ 32″	17° 57′ 33″
Freemantle, Lighthouse	115 44 23	32 3 14
Albany, Wakefield Pt.	117 53 41	35 2 0

and "it would appear that all longitudes on the western coast of Australia ought to be reduced by 48″; but those from N.W. Cape to Roebuck Bay require to be altered half that amount only."

MATHEMATICAL RECREATIONS AND PROBLEMS*.—Mr. Ball, to whom we are already indebted for two excellent Histories of Mathematics, has just produced a book which will be thoroughly appreciated by those who enjoy the setting of the wits to work, even on what may appear to be trifling matters. He has collected a vast amount of information about mathematical quips, tricks, cranks, and puzzles—old and new; and it will be strange if the most learned do not find something fresh in the wonderful assortment. The book has not much to do with astronomy, though there is a chapter on astrology; but many astronomers have in their leisure moments puzzled over magic squares, the knight's path on a chessboard, or the 15 schoolgirl problem; Chinese rings or mazes and labyrinths; card tricks and geometrical fallacies: and to such we can heartily recommend Mr. Ball's book. We make bold to quote a most amusing geometrical fallacy from p. 32.



Let ABCD be a square. From A draw a line AE equal to AB and outside the square, making an acute angle with AB, as indicated in the diagram. Bisect CB in H, and through H draw HO at right angles to CB. Bisect CE in K, and through K draw KO at right angles to CE. Since CE and CB are not parallel, the lines HO and KO will meet (say) at O.

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* 'Mathematical Recreations and Problems of Past and Present Times.' By W. W. Rouse Ball. (Macmillan and Co., 1892.) Price 7s.

Join OA, OE, OC, and OD.

The triangles ODC and OAE are equal in all respects. For, since KO bisects CE at right angles we have OC=CE.

Similarly, since HO bisects CB and DA and is perpendicular to them, we have OD=OA.

Also, by construction, DC=AE.

Therefore the three sides of the triangle OAE are equal respectively to the three sides of the triangle ODC.

Hence, by Euc. i. 8, the triangles are equal, and the angle ODC is equal to the angle OAE.

Again, since HO bisects DA at right angles,

the angle ODA = the angle OAD;

hence the angle ADC = the angle DAE.

But ADC is a right, and DAE is necessarily greater than a right angle.

Thus the result is impossible.

PREPARATION AND DISCUSSION OF THE DRAPER CATALOGUE.—We noticed in the number of the 'Observatory' for last October the appearance of the great 'Draper Catalogue.' And now the energetic Director of the Harvard College Observatory has issued a companion volume to it, in No. xxvi. Part I of the 'Annals' of that Observatory. The object of this second work is to furnish such a history of the methods employed in the formation of the 'Draper Catalogue,' and such additional details respecting the photographs taken for it and their measurement and reduction, as could not conveniently be given in the Catalogue itself. These various points occupy the Introduction,—which gives a history of the whole progress of the Draper Memorial, passing in rapid review its leading triumphs, such as the discovery of the duplicity of β Aurigæ and ζ Ursæ Majoris, and the remarkable variations in the spectrum of β Lyræ. Of the first four chapters following, the first is devoted to the instrument employed, the Bache telescope, the second to a catalogue of the 3300 plates taken with it: these were of different kinds and taken for different purposes, a large proportion not being taken directly for the Catalogue, but for various experimental purposes; the third chapter is devoted to their classification. The methods employed in the measurement and reduction of the plates are described in the next chapter, and then follow five chapters of especial interest, of which two relate to those "photographic magnitudes" which formed so important a feature in the great Catalogue, and two to the distribution of the various types of stellar spectra, and to a comparison between the classification as effected by photography, and those made by visual observation by Vogel and Konkoly. Amongst the points brought out in these various discussions are the predominance of first type spectra in the Milky Way, and the systematic undervaluing of the brightness of the galactic stars, by about one fifth of a magnitude, by the 'Durchmusterung' and 'Uranometria Argentina' as compared with the Harvard photometric and photographic magnitudes. The comparison with the catalogues of Vogel and Konkoly leads to the conclusion that the latter observer did not adopt quite the same criterion for first type stars as is generally done.

Yet a third volume is to follow, to be devoted to the work of the 8-inch Draper telescope during the years 1889 and 1892 and to the discussion of stars of peculiar spectrum, and to render yet further complete the magnificent monument to Dr. Draper's memory which has thus been erected by the liberality of Mrs. Draper, and the skill and energy of Prof. Pickering and his assistants.

THE Royal Meteorological Society has recently published a new edition, the third, of their 'Hints to Meteorological Observers,' * as prepared, under the direction of the Council of the Society, by Mr. W. Marriott, the Assistant Secretary. This useful little manual gives instructions, designed principally for amateur observers, in regard to the placing of meteorological instruments, the precautions to be attended to in their use, with plain directions for the making of meteorological observations. Numerous illustrations of meteorological instruments are also included. There is likewise appended a useful collection of meteorological tables. We note that the description of the Rain-band Spectroscope, previously included, has disappeared from the present edition, which we presume indicates the degree of estimation in which the instrument is held by meteorologists generally. A description of Jordan's sunshine recorder is given: it is a cheaper but not so satisfactory a form of instrument as the Campbell-Stokes arrangement, that more generally used.

NOTES.

SPOTS AND FACULÆ.—Some time since Father Perry stated in a lecture at the Royal Institution that no clear instance of faculæ preceding the birth of a spot had been detected in the Stonyhurst drawings. Father Sidgreaves announces in a recent paper ('Monthly Notices,' vol. lii. no. 2) that amongst the drawings of the recent minimum period of 1889 have been found two cases in which the evidence of first appearance of a disturbance in a particular region is unquestionable, and in both of which faculæ are shown before any trace of a spot appears. The point is one of considerable interest, and we hope for further information on the subject at an early date.

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* 'Hints to Meteorological Observers.' Price 1s. Stanford, Charing Cross.

THE following is an approximate list of occultations visible at Greenwich during the almost total eclipse of the Moon on May 11. Only phenomena at the eclipsed limb are included.

Star's Name.	Mag.	Time.	Dis. or Reap.	Angle from N. Pt. towards E.
B.D. −19° 4087	9·7	$9^{h}\ 45^{m}$	D.	142°
−18 4047	9·0	9 49	D.	65
−19 4091	8·3	10 16	D.	112
„ 4093	9·2	10 20	D.	94
−18 4047	9·0	10 26	R.	355
−19 4095	8·9	10 33	D.	90
„ 4096	9·4	10 43	D.	103
„ 4087	9·7	10 58	R.	281
„ 4097	9·4	11 10	D.	57
„ 4099	8·2	11 12	D.	109
„ 4093	9·2	11 31	R.	327
„ 4091	8·3	11 34	R.	309
„ 4095	8·9	11 42	R.	332
„ 4097	9·4	11 43	R.	8
„ 4096	9·4	11 59	R.	317
„ 4099	8·2	12 31	R.	306

The star 4099 is a double star, the components being nearly equal and about 4″ apart.

COMET NOTES.—Haerdtl has published (Ast. Nach. 3083) a continuation of the ephemeris of Winnecke's Comet till Sept. 22:—

Ephemeris for May.

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
May 1....	11	32	42	44	8	May 21 ..	11	5	19	44	17
5....		26	20	44	26	25 ..		0	41	44	0
9....		20	27	44	34	29 ..	10	55	50	43	38
13....		15	3	44	35	June 2 ..		50	23	43	12
17....		10	3	44	29	6 ..		43	46	42	41

The comet will be at its brightest early in July, but unfortunately its proximity to the Sun will prevent its being observed then.

Lamp publishes the following ephemeris of Swift's Comet:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
May 1....	22	32	49	20	45	May 17 ..	23	17	27	30	39
5....		44	36	23	31	21 ..		27	33	32	41
9....		55	58	26	6	25 ..		37	13	34	33
13....	23	6	55	28	28	29 ..		46	25	36	18

The comet continues bright, and its increasing North declination will render it easy of observation.

Ephemeris of Denning's Comet for May.

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
May 1	2	44	45	56	50	May 13....	3	29	7	53	57
5	3	0	37	55	54	17....		41	55	52	57
9		15	23	54	57	21....		53	52	51	55

The brightness throughout the month is slightly less than that at discovery.

Continuation of Gautier's search Ephemeris of Tempel's Comet (from Ast. Nach. 3075).

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
May 5....	18	24	33	23	46	May 25 ..	18	24	42	26	24
10....		25	53	24	22	30 ..		22	42	27	6
15....		26	21	25	2	June 4 ..		19	59	27	48
20....		25	58	25	43	9 ..		16	43	28	30

There is a probable error of some 30^{m} in R.A., 1° in Decl.

Nos. 3084 and 3085 of the Ast. Nach. are taken up with an elaborate discussion by Auwers of the orbit of Sirius, the observations used extending from 1751 to 1864. Using Gill and Elkin's value of the parallax (0″·38), the mass of Sirius comes out 2·20 times the Sun, and that of the companion 1·04. The semi-axis major of the relative orbit is 19·92, or about equal to that of the orbit of Uranus. A table is appended giving the corrections for orbital motion to the R.A. and N.P.D. of Sirius from 1850 to 1900.

Minor Planets.—The planet numbered 323 in the January number has been found identical with Urania (30), so that for this year the record stands:—

323 Jan. 20 (Wolf) ; 324 Feb. 25 (Palisa);
325 March 18 (Wolf) ; 326 March 19 (Palisa);
327 March 22 (Charlois); 328 March 26 (Wolf) ;
329 March 28 (Wolf) ; 330 April 1 (Charlois).

The following minor planets have been named:—

305 Gordonia; 307 Nike; 310 Margarita.

The Astronomical Exhibit at Chicago in 1893.—The following note is taken from 'Astronomy and Astro-physics':—"The scope and nature of this exhibit will largely depend upon the liberality with which astronomers and instrument makers respond to the call for a full and complete display. Advices already received from Warner and Swasey, J. A. Brashear, and Alvan Clark indicate that there will be no lack of instruments of the highest class. It is hoped that there will be at least one refracting telescope of fully 20 inches aperture, and two exceeding 12 inches. Reflectors will be shown in various sizes, while the mere fact that Brashear will exhibit is sufficient guarantee that spectroscopes of all kinds, gratings, prisms, flat surfaces, &c., will not be lacking. Two large domes have been arranged for, and a complete working model of the Lick Observatory is now being made.

At the Lick Observatory a large number of transparencies on glass, eight inches by ten in size, are being prepared from negatives of the Moon, Jupiter, &c., and the remarkable success of the Henry Draper Memorial will no doubt be exemplified by a large collection of photographs of the stars and stellar spectra from the Harvard College Observatory. It is to be hoped that Prof. Rowland will send many specimens from the extensive series of photographs of solar and metallic spectra on which he is now engaged. It will be noticed that American exhibits only are here called for. The arrangements of the 52 foreign countries which have officially announced their intention of participating in the Exposition are such that the exhibits will be grouped by nations, rather than by subjects. While this natural system may possess some disadvantages as compared with a rigid classification by subjects, it will at the same time have the corresponding advantage of stimulating national pride. If, as we hope, foreign countries will give as much attention to an astronomical as to an industrial exhibit, the United States will need to look to her laurels."

We regret to record the death of Professor Annibale De Gasparis, Director of the Observatory, Naples, which took place on March 21. He was the son of a Doctor of Medicine and was born on November 9, 1819, at Bugnara in the province of Aquila, his youth being passed at Tocca Casauria. In 1838 he went to Naples to study mathematics and astronomy under Professors Tucci and De Angelis. In 1842 he was appointed assistant to Professor Capocci at the Capodimonte Observatory. While there he employed his time in making charts of the ecliptic in the course of a systematic search for minor planets and discovered Hegeia in 1849; Parthenope and Egeria being discovered in the following year.

At this time three out of the thirteen known minor planets had been discovered by De Gasparis. For this work and his investigations of the orbits of planets he was elected a Foreign Associate of the Royal Astronomical Society on Dec. 13, 1850, and the Gold Medal of the Society awarded to him in February, 1851, the address being delivered by the late Sir G. B. Airy, the then Astronomer Royal. In 1851 De Gasparis was appointed Professor of Astronomy at the University at Naples, and on the death of Capocci in 1864, he succeeded to the post of Director of the Observatory, which office he held until his death.

Between 1849 and 1865 nine minor planets were discovered by De Gasparis, and since the latter date he has devoted considerable time and attention to the orbits of minor planets, to Kepler's problem, and to the theory of perturbations.

He was a frequent contributor to scientific societies and astronomical journals, a list of 151 of his pa before 1883, being given in the Roya Scientific Papers. He was one of the 40 memb d'Italia and a member of most of the Italian scie well as of numerous foreign societies. He was elect 1861. With his numerous honours he still preserve and rare simplicity, and was the same earnest worker health failed several years ago. He died, beloved and ven his numerous students and colleagues, on March 21.

We regret to announce a fatal accident to Mr. John Har Astronomer to the Mersey Docks and Harbour Board, who killed on April 21 by falling from the top of the observatory Bidston Hill, Birkenhead. Mr. Hartnup was born in 1841 at th old observatory, Liverpool, and succeeded his father, who was f some time Assistant Secretary to the Royal Astronomical Societ as Astronomer to the Mersey Docks and Harbour Board in 1885. The work at the observatory is chiefly meteorological observations and the rating of chronometers, and while inspecting the anemometers on the roof of the observatory, Hartnup was seized with giddiness (with which he has been troubled for some time past) and fell over the parapet wall, being instantly killed.

At the inquest a verdict of accidental death was returned, the jury adding that the wall was too low (only 20 inches in height) for the purpose for which it was erected. The principal work of Hartnup is recorded in the publications of the 'Bidston Observatory.' He was elected a Fellow of the Royal Astronomical Society in 1886, Feb. 12. He was also a Fellow of the Royal Meteorological Society and of the Liverpool Astronomical Society.

Mr. G. Comstock, of the Washburn Observatory, has been engaged during the last year on a determination of the Constant of Aberration and sends a preliminary paper to the 'Astronomical Journal.' The method he used was that suggested by Loewy of placing 3 reflectors in front of the object-glass so as to combine stars in different parts of the heavens. He used 3 reflectors, bringing together stars distant from one another 120° and compared their computed and measured distances, each pair of reflectors being in turn employed for measuring the distance between each pair of stars. He discusses both the refractions and aberration constant indicated by his observations, arriving at values very close to the Pulkowa tables for the former, and $20''{\cdot}494 \pm 0''{\cdot}017$ for the latter.

We learn from 'Ciel et Terre' that from the 1st of May next the Belgian railways are to adopt Greenwich Time. The 'Annuaire,' which is published at the Brussels Observatory, will also be adapted to Greenwich. Also in Holland the change will take place on the 1st of June.

THE OBSERVATORY,

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No. 189. JUNE. 1892.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, May 13, 1892.

E. B. KNOBEL, *President*, in the Chair.

Secretaries: H. H. TURNER, M.A., B.Sc., and E. W. MAUNDER.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Maunder. Seventy-six presents have been received since the date of the last Meeting. Those calling for special mention are:—Original negatives of Jupiter, taken with the Great Telescope of the Lick Observatory, presented by the Lick Observatory; Original negative of Swift's Comet (1892), taken at the Sydney Observatory, presented by Mr. Russell; Positive on glass from negative of the total Solar Eclipse of 1871, taken in Java, presented by Prof. Oudemans; R. Wolf, 'Handbuch der Astronomie,' 3rd half-vol., presented by the Author; Oxygen-bottle for use with the lantern at evening meetings, presented by Mr. Ristori. Later on an enlargement on glass from a negative of the region surrounding η Argûs, by Dr. Gill at the Cape Observatory, was presented in the meeting by the President.

A vote of thanks was passed to the donors of the presents.

Mr. Turner. Mr. Russell sends us two photographs of the Comet Swift—one an original photograph, and one a photograph of a drawing. Unfortunately the latter got broken in transit. The comet negative is an exceedingly beautiful one, and Dr. Spitta will endeavour to show it on the screen. The photograph was taken on 1892, March 22, and is one of the best, although there is a better one, which Mr. Russell naturally kept at his observatory. In the photograph of March 11 the rays in the tail of the comet can be seen extending quite to the edge of the plate, whereas on that sent to us the rays are not quite so long. Nevertheless they are very striking. The plate has a réseau on it, and is in fact one of the Chart plates; so that the dimensions of the comet can be clearly made out. The réseau can be seen on the screen, and the length of the trails of the stars will be noticed as showing the very rapid rate of movement of the comet.

[The original negative was projected by means of the microscope attached to the lantern.]

The President. Is there any confirmatory eye-observation of these remarkable lines in the tail of Swift's Comet? I was able to observe this comet myself by eye in the Southern Hemisphere. The length of the tail is remarkable, and I have not seen it noted in any journal. On 1892, April 1, it was about 8° long; on April 2 it was 12°; and on April 4, 14° or 15° in length; but the comet had not increased in brightness during this time, in fact it had rather diminished. I should like to say a word also about the position of the comet. I notice that the published ephemeris is given as having an error of 2° in declination. On April 1, although I was at sea and had only a small binocular, I was able to observe the comet as being close to a small but well-known star (about 7′ or 8′ preceding Lalande 40345), which would indicate a close agreement with the comet's place in the ephemeris.

Mr. Turner. I am sure, Sir, the meeting will agree with me in one matter, although it has nothing to do with the comet. We are extremely glad to see you back again amongst us after your long sea-voyage.

A vote of thanks was passed to Mr. Russell for his paper.

Father Sidgreaves gave the substance of a paper "On the Spectrum of Nova Aurigæ."

I ought to say at the beginning that I am entirely indebted to Dr. Huggins for the photographs of the spectrum of the temporary Star in Auriga; and this on two grounds:—first, that without his kind message to me on the 2nd Feb. I should have missed the only favourable opportunity of its greater brightness on the clearest night of the 3rd; and, secondly, that but for his request my previous experience with β Lyræ would have led me to decide that it was useless to try my optical power on so small a star. But, to be able to say that I had done all I could, I adjusted the clock-rate to give as slow a trail of the spectrum over the plate as I could venture, and left it for 1¾ hours. My surprise was very great to find so strong an impression on the plate.

The most striking features of the photograph as it appears on the screen are the great width of the hydrogen lines, and their dark companions. It seems quite impossible to satisfy one's self that the dark bands are mere effects of contrast. But the width of the bright lines needed further investigation. The length of exposure might have given width to the lines, by bringing out the effects of scintillation upon a plate not protected by a slit. For this purpose I recently exposed two plates to γ Cassiopeiæ at its lowest altitude, while its scintillations were very marked. The first of these, with a moderately slow trail, showed all the details of the spectrum with definition in no way inferior to my best plates of the star when close to the zenith; and on the second plate, with a trailing rate even slower than was employed on the Nova, no appreciable widening of the bright F line was perceived.

These plates are not strong enough for projection upon the screen; but I have with me two negatives of α Lyræ, one of which was exposed with an excessively slow trail, and these will illustrate conversely the experiments on the bright lines of γ Cassiopeiæ. The very slow trail on α Lyræ shows the absorption-lines of hydrogen thinner than the bold lines of the quicker trail. On the slower trail the absorption-lines have been contracted by the scintillating encroachment of the bright margins of the continuous spectrum; and the effect is similar to the obvious widening of the bright lines at the middle of the trail on the Nova when the clock altered its rate and kept the star stationary for a time. Apparently, therefore, only excessive slowness of the trail has any appreciable effect on the width of the lines.

The map of the spectrum shown on the screen needs some explanation. It is a double map of the same spectrum. The lower one shows the lines or bands as they are easily recognized by any observer examining the plate with a suitable magnifier; and the upper one is the result of a very careful and prolonged study of the two plates exposed on the night of Feb. 3. There are many more lines in this map, both bright and dark; and every line has the authority of two independent examiners, both of them experienced assistants of the late Father Perry, viz. Father Cortie and Mr. W. McKeon. No line has been admitted that has not been detected by each examiner on both plates.

The wave-lengths assigned to the lines were taken from an interpolation-curve set out from 42 lines of Capella identified as solar lines, and fairly evenly distributed between D and H. The curve appears to be very satisfactory, running quite smoothly through all the fiducial points. The scale-readings are those of a firmly-mounted micrometer-screw and magnifier by Hilger, reading to $\frac{1}{1000}$ of a millimetre. All the wave-lengths, therefore, are referred to the adopted position of some one known line of the Nova. The line selected was that of the bright hydrogen at F, and the middle of this, regardless of its probable subdivision, was taken for the wave-length number 4861.

The object of my paper is rather to give a faithful account of the work connected with the map and catalogue of lines than to offer any theory of my own about the origin of the wonderful spectrum offered to our study, in order that the Fellows may be in a position to form their own judgment upon them when presented to the R.A.S. But one observation has forced itself upon us while taking off the wave-lengths from the curve. The lines of the Nova nearest to the Capella lines of the curve fell to wave-lengths a little short of the Capella lines. And since it was highly probable that the line F of the Nova was a double, it became also probable that the more refrangible of the pair had the better claim to the w.l. 4861. And when the map was complete it appeared that if this shift of the fiducial point of the map were made, many of the lines would be brought into close agreement with the solar chromospheric lines of Professor Young's Catalogue. The subject needs further consideration, and may be treated in another paper.

The method of photographing stellar spectra followed at the Stonyhurst Observatory is the one known as the trailing method without a slit—the same as is employed at the Harvard College and South Kensington observatories. But I have not the advantage of an objective prism, which gives so much more available light. My spectrograph is a small direct-vision prism, with its collimator and image-lens at the eye-end of the telescope. The instrument was constructed by Mr. Hilger with two prisms for my lamented predecessor, to form a part of his equipment for one of the Solar Eclipse expeditions. One of the prisms has been removed, and I have obtained better results with this instrument than with other spectroscopes belonging to the observatory. I have been driven to give up successively the use of the cylindrical lens and of the slit. The first was put aside because I could not afford to stand by, all the time of a long exposure, to keep the star's image from wandering away from its place on the slit; and the second was found to be a practical impossibility, owing to the necessity of dismounting for day-work and the consequent difficulty of a satisfactory re-orientation of the slit to secure the star's safe journey along it.

The President. I think Father Sidgreaves is to be congratulated upon having secured such a valuable record of the beautiful appearance of the spectrum of Nova Aurigæ. I will ask you to return your thanks to him for his paper.

We are honoured to-night by the presence of our distinguished American Associate and Medallist, Professor G. W. Hill. In the name of the Society, I offer him a hearty welcome. [Cheers.] I am sure I express the feelings of all Fellows when I say that if he would give us any remarks on his researches in Physical Astronomy or the Lunar Theory they would be very much appreciated by the Society.

Prof. G. W. Hill. I fear I have somewhat neglected the Lunar Theory for a good many years, and have been engaged on other work, especially on Jupiter and Saturn; but since the President has referred to my earlier researches, I may say I think I should perhaps have done better if I had stuck to them. And I will say a very few words about the Lunar Theory. It seems to me that there are some periodic inequalities which we have not fully worked out yet. Probably also there may be improvements to be made in the terms with which we are at present familiar; and it will take many years to completely work these out. Some of the new terms may require fifty years, some perhaps three or four hundred years, to have a sensible effect; but they are important, although very small. I am very much pleased to see that M. Radau at Paris has taken up this subject, and given us a new and important article on it. He is, I believe, about to contribute some very important results on this subject. Another very important operation in the study of the Lunar Theory is the determination of the coefficients with a higher degree of accuracy than has hitherto been made. I know it is a matter of great difficulty, and I am glad to hear that Mr. Ernest Brown, of Cambridge, has taken up the labour which I have left off for so many years. In the last number of the 'Journal of Mathematics' mention is made of his work, and some of the results he has obtained are exactly the same as those which I have myself obtained but not yet published. I am afraid I have nothing very new to say upon this subject; but I am very much obliged to you, Mr. President, for the opportunity of making this short statement.

The President. I am sure our thanks are due to Professor Hill for the few words he has addressed to us. We have also present among us to-night another distinguished American, Mr. Brashear, who is so well known for making some of the finest instruments, especially for spectroscopic work, in the world. I hope he will give us an account of his work, especially of any new instrument he is constructing.

Mr. Brashear. It affords me a great deal of pleasure to meet so distinguished a body as the Royal Astronomical Society, whose work I have known about almost from my childhood, and in a little of which I have been associated. I think I cannot do better than try to say something which will be of value to astronomers. Father Sidgreaves was speaking of the dust-lines on the spectrum, and I have accidentally made a little discovery about these.

My dear old friend Mr. Dallmeyer wrote to me asking me to find out something that would clean the dust out of the spectrum, and I found that a clean quill toothpick, which I had in my mouth, was one of the most magnificent things to get the dust-lines out of the spectrum. I found nothing was so good as a soft quill, and I advise you to buy sixpenny worth of quills and cut them up into toothpicks, and always have one in your mouth when you want to get the dust-lines out of the spectrum. I have with me a picture of the last spectroscope we have made for Mr. C. A. Young. It is the most complete one that has been constructed up to the present time. Mr. Bowditch gave him the money and told him to get as good a spectroscope as he could, and this combines everything one can desire. The new star in Auriga, however, came along before it was ready, and then we found that there was a little irregularity in the prism. Thus Father Sidgreaves and Mr. Pritchard got ahead of him, and he does not like it. We constructed one for Mr. Keeler, of the Alleghany Observatory, who is a very promising young man. The work of astronomy in astro-physics is going on nicely, and I believe the public spirited and monied men in America are coming up to help the poor astronomer. I heard of a gentleman just before I came away, who said he would give a large sum to build a new observatory. We have a gentleman present who has contributed largely to the great Denver telescope, and a new observatory for Washington has been contracted for. All over the United States there is great interest being shown in astronomy, and particularly in new astronomy. Mr. Hale has been doing excellent work, and the last instrument we are making for him is a very complete one indeed. We are finishing a photographic objective with a 12-in. aperture of exactly the same focal length as the visual objective, and the two will be mounted like spectacle-lenses, so that he can swing them round and use either in the same tube. It is most enjoyable to me to meet such a distinguished class of gentlemen who have done so much for science and for pushing out the borders of human knowledge. I thank you very much for your kindness.

The President. I am sure we shall all thank Mr. Brashear for his interesting communication. We are all indebted to him for the great services he has rendered to astronomy by the excellence of the instruments he has constructed.

I would desire at this time to present to the Society a photograph which was taken for me the other day at the Cape of Good Hope, in remembrance of my visit to Dr. Gill. It is a photograph of the nebula round η Argûs, taken by Dr. Gill with $3\frac{1}{4}$ hours exposure with the astro-photographic telescope. I think it will be found to be one of the finest photographs of a nebula ever produced. By counting selected fields on the plate, it has been estimated that over 40,000 stars are shown.

Mr. Turner. I would ask the Society to return thanks to our President and to Dr. Gill for this beautiful photograph.

Prof. R. Copeland read a paper "On a pretended Early Discovery of a Satellite of Mars." This was the title of a note in which a short account was given of a bibliographical curiosity entitled 'Eberhard Christian Kindermann . . . Astronomische Beschreibung und Nachricht von dem Cometen 1746. Und denen noch Kommenden, . . . 8 ff., 1 plate. 4°, Dreszden, 1746.' In it Kindermann gives a few doubtful observations of the comet of February 1746, of which the very existence is somewhat uncertain, although Dr. Hind has succeeded in deriving a rude orbit from the alleged observations. Kindermann regards the object described by him as identical with the comet of 1682, of which he had predicted a return for the year 1746. He furthermore tried to show that the comets of 1532 and 1661 were one and the same, and might be looked for again in 1790. It may be mentioned that the identity of these two comets was afterwards repeatedly surmised by computers, but had eventually to be abandoned. The great comet of 1744 he also considered likely to return in 1757. The imaginary paths of these comets are laid down in the frontispiece, in one corner of which is inserted a diagram professing to give the orbit of a satellite of Mars discovered by Kindermann two years before.

The encircling legend runs "*Via Luna* (sic) *Martis entdecket vom Autore den* 10. *Iul.* 1744." Both satellite and primary are surrounded by dense atmospheres. The object figured by the author is in all probability simply a "ghost." It will be noticed that the date of the tract is intermediate between the appearance of 'Gulliver's Travels' and 'Micromegas,' with their circumstantial accounts of the moons of Mars. A copy of the tract was presented to the Society in the name of Lord Crawford.

A vote of thanks was accorded to Prof. Copeland for his paper.

The Astronomer Royal. I have brought before the Society to-night a suggestion for a new form of altazimuth. It is really a transit-circle to be used in any given azimuth, and so arranged that it can be transferred from one azimuth to another, although that operation would not, as a rule, take place in the course of one evening. The model I have brought, which was designed by Mr. Simms, will show the general principles of the instrument without entering into great detail. We have, in the first place, a reversible transit-circle of Messrs. Troughton and Simms' well-known form, the object-glass of which is 8 inches in aperture. This is mounted on the usual iron supports, but the means for raising and reversing the instrument have to be specially adapted to this case. The supports are fixed on a base mounted on a turn-table, so that it can be set to a definite azimuth and firmly fixed there. Where this instrument differs from the ordinary altazimuth is that, instead of being used in any azimuth which may happen to be fixed upon by chance in pointing to an object, it is fixed in a definite azimuth and used for a series of observations; and so long as it is in that azimuth, it may, I think, be treated as a transit-circle. This is so both as regards stability of position and for the reductions required for observations of zenith-distance and azimuth. The N.P.D. and R.A. of an object are easily obtained, and the reductions are much simplified by the fact that the instrument is used in a definite azimuth, for which various auxiliary quantities can be calculated. The general principle of the instrument is that it is essentially a transit instrument used out of the meridian. I will not go fully into the principles involved in using the instrument, but will briefly indicate them. In the altazimuth as ordinarily used, the observations of zenith-distance and of azimuth are made independently. The azimuth is first observed, then a horizontal transit is taken, and the zenith-distance circle read. It is usual to set to two different azimuths, and to observe zenith-distances by taking the time of transit across a series of horizontal wires. The two sets of observations, those for azimuth and those for zenith-distance, refer to different times, and each has to be worked out independently. In the new instrument, I propose still to take transits across horizontal wires and vertical wires, but to get the means at the same time, so that we shall get the azimuth and zenith-distances at very nearly the same instant of time. Before the object comes into the field we can tabulate the parallactic angle for the azimuth and N.P.D., at which we are using the instrument. This can be approximately done, and the angle does not vary much with the N.P.D. under ordinary circumstances. Then, knowing the path of an object, we can, by means of a supplementary wire mounted on a position micrometer, arrange so that the object will transit approximately across the intersection of the two middle wires of the network used. If there is a small error, and the star does not pass the intersection, it will be easy to find this error, and allow for it in the reduction of the zenith-distance from our observations. I think that this process will be found to simplify the calculations very much, and to enable us to deal with a large number of observations without the very great labour which is involved in reducing ordinary altazimuth observations. The main part of the formulæ will consist of constants, and we shall have only one trigonometrical function to deal with, so I think it will be found comparatively simple in practice. I will now briefly deal with the determination of the instrumental errors. It will be understood that from observations of stars with this instrument we get the N.P.D. and hour-angle, which with the clock error will give the R.A. If we know the R.A. of clock stars we can get the clock error, so that the reductions will be the same in that particular as for the transit-circle. For instrumental errors of zenith-distance we can find the zenith point by nadir observations, the R − D correction being determined by reflection observations of stars with the transit-circle in the meridian.

The collimation error would be determined by collimators in the meridian (or in the prime vertical), and it would also be determined by reversing on the nadir. As regards error of level, it would be found from the nadir observations by reflection.

The azimuth would be given by two observations of a circumpolar star (Z.D. and time of azimuthal transit) at upper and lower culmination, the colatitude being known, or by a single observation if the N.P.D. as well as the colatitude be known.

Now the question comes as to the uses to which we can put this instrument, and the importance of it in determining fundamental positions with accuracy. It can be used for observations of Moon and planets when the Moon or planet is in such parts of its orbit that it cannot be observed conveniently on the meridian or if it passes at an inconvenient hour in the morning. With an instrument like this an observer would be saved a great deal of labour and of waiting, for the planet could be observed three or four hours before or after the time of meridian passage. It will be a point of special importance to observe the crescent Moon or the planet Mercury out of the meridian, and when they are too near the Sun to be observed with the transit-circle on the meridian. Then, again, for determining what may be called astronomical constants, the latitude, constant and law of refraction, systematic errors of N.P.D. and the various quantities about which there are now so many controversies; it will be of great value to get independent determinations of these under different instrumental and observational conditions. I hope that an instrument of this sort will throw a great deal of light upon these vexed questions. Although the instrument itself has not been carried out, I thought it would be interesting to bring forward the idea and show the model which Mr. Simms has made in accordance with it.

Prof. G. W. Hill, I should like to know whether the instrument will possess sufficient stability. The question is, would not the turn-table be the cause of producing some shakiness? It has been a very difficult problem in the past to construct altazimuths that are sufficiently stable to give perfect results.

Father Sidgreaves. I think Sir George Airy said that he valued an instrument more the less adjustments it had, and that the perfect instrument would have no adjustments whatever. He liked simply to take an instrument as it stood, to find its errors and allow for them in the computations.

Mr. Turner. I should like to be allowed to express my interest in the new instrument. Among our computation forms for the altazimuth at Greenwich is one very much dreaded by new computers and known as Form 72. There is a grim legend about two computers who are "in the asylum now" in consequence of this Form. I do not think the Astronomer Royal intends to altogether get rid of Form 72, but he will diminish its virulence; and in many ways we look forward with great interest to the new instrument.

The Astronomer Royal. Father Sidgreaves and Prof. Hill remind me that I have not entered into sufficient detail. First, as regards stability. This instrument is mounted on a turn-table, but it is an essential condition that, although friction-rollers are brought into use to turn the instrument as soon as it is turned into a definite position, it is allowed to rest with the whole of its weight, which may be perhaps a ton or more, upon the bearing, so that the instrument will be substantially stable and fixed. There will be a perfectly plane surface, and the instrument will bear on three portions of that surface. I had proposed that it should be bolted down firmly, but I do not know if that will be necessary or not. I think that satisfies the condition that the instrument should be absolutely without adjustment when in use. It will be brought as near as possible to the required azimuth, but I suppose that it will have a small error, with which we shall be able to deal in the usual manner. I do not like the ordinary form of central cube to the tube with perforations. With the transit-circle we have had a great deal of trouble from the interrupted view through the cube, and I think that the perforation is objectionable. It weakens the instrument at its weakest point, and allows of the possibility of unequal flexure in the two halves. I think there is always some objection to making a comparatively large hole exactly where you want most strength, and so, with Mr. Simms' concurrence, I propose that this should not be pierced in the centre, but should be quite continuous and barrel-shaped. I propose also that the object-glass and eye-end should be absolutely interchangeable, so as to eliminate any differences of flexure between the two ends. I do not claim any novelty in this, it has been suggested before; but I mention it because it was in my mind when I devised the instrument. Then another point raised was the inclination of the wires; but I think we have means for determining that. I should propose to determine the positions of the wires in the meridian with the collimators. That is a thing that should be determined with very great accuracy. In using this transit-circle out of the meridian it is obvious there will be a number of small problems which will have to be worked out by the light of experience. So far as my proposal goes, it is that the instrument should be absolutely fixed whenever it is used in whatever position out of the meridian, and that it shall not be liable to any adjustment as regards azimuth.

The President. What are the proposed dimensions?

The Astronomer Royal. I propose an 8-inch object-glass of 8 ft. focal length, with a 3-foot circle. As to stability, I think astronomers have had exaggerated notions as to the stability required in an instrument and as to the stability which the so-called solid earth gives us. My experience has been that when you have to deal with masses of metal the movements due to instability of the soil are greater than those you will have to apprehend in changes of the instrument itself. We may have variations of several seconds in solid stone piers arising simply from a change of temperature. In fact, we have found that our transit-circle at Greenwich, mounted on stone piers which have been standing for nearly half a century, is substantially a thermometer, and you can detect changes of temperature by changes of level of the instrument, a change of temperature being always accompanied by a change of level. When we have such changes as these in the soil, I do not think we need be much afraid of the small change in the bearings of one heavy casting upon another when they are resting upon dead bearings.

A vote of thanks was passed to the Astronomer Royal for his paper.

Mr. Isaac Roberts read a paper on a "Photographic Search for a Planet beyond Neptune." He said that Prof. Forbes, in a paper read before the Royal Society of Edinburgh in 1880, predicted with considerable confidence that one or two planets existed exterior to Neptune, and very full reasons were given in the paper. The prediction was based on the recorded positions of the aphelia of a number of comets, and the coincidence of these led Prof. Forbes to infer that the existence of these planets could be no longer in doubt, and that their orbits were respectively at about 100 times and 300 times the distance of the Earth from the Sun. In 1887 Mr. Roberts wrote to Prof. Forbes, the author of the paper, for further particulars, and he replied, giving the predicted position of one of the planets in R.A. $11^h\ 48^m$ and Decl. 3° north, and stating that he believed a range of 5° in R.A. and of 2° or 3° in Declination from the places given ought to find the planet if it were there. The motion of the planet Prof. Forbes computed at 1° in 2·96 years. A chart of the region in question was consequently prepared at Maghull, but the climate there did not allow the search to be made. At Crowborough, however, complete photographic charts had been made of the region between R.A. $11^h\ 24^m$ and $12^h\ 12^m$, with declination from the equator to 6° N. This region was covered by 18 photographic plates, each taking in 4 square degrees in area, and allowing sufficient overlap to prevent any stars being missed. The photographs were taken in duplicate at intervals of not less than 7 days between the exposures, which were each of 90 minutes duration. The whole of the plates covering the region were very carefully examined, and it only remained for Mr. Roberts to report that no planet of greater brightness than a star of the 15 mag. existed in the sky area indicated, nor was there anything on the plates of an abnormal appearance to which it was necessary to call special attention. It was a region in which stars were not exceptionally numerous, and they were mostly faint.

Mr. Turner. Would any minor planets have been shown on the plates?

Mr. Roberts. Yes. With 90 minutes' exposure there would have been a comparatively long trail equal at least to six times the photographic diameter of a minor planet, if one had been present during any of the exposures of the plates.

Mr. Roberts then read a paper on "Photographs of the Region of the Crab Nebula: 1 M Tauri." Two enlarged photographs were presented, one to the scale of one centimetre to four minutes of arc and the other to the scale of one centimetre to sixty minutes; the latter showed the nebula to be of an oval form in general outline, but not quite symmetrical, with the major axis in north preceding to south following direction. There is no indication of the filamentous projections round the nebula which some drawings show, and if they had an objective existence the photograph, with the long exposure of three hours, would undoubtedly have shown them. The photographs were also shown projected on the screen by the lantern.

The President. Mr. Roberts's communications are always interesting, because they always advance our knowledge. The photograph of 1 M is very interesting, though I think it is not unlike the drawing by Secchi. I can also trace a resemblance to the well-known drawing by Lord Rosse, though the filaments are not shown on the photograph which are shown on the drawing. Perhaps a shorter exposure might have revealed some fainter outlines which form part of the details of Lord Rosse's drawing.

Mr. Roberts. I have taken photographs of this nebula with short exposures as well as long ones Of course the longer the exposure the more of the fainter nebulosity will be shown. The nebulosity in the Pleiades, for instance, will be shown in 90 minutes, and with the photographs shown to-night, having an exposure of three hours, nothing so faint as the nebulæ in the Pleiades would be omitted.

Lord Rosse. There are three drawings of this object in my book. One is of the nebula with filaments sticking out, like a soldier crab; this was made with the 3-foot, and at that date the object in view was to see how far nebulæ could be resolved into stars rather than to make finished and accurate sketches. The other two, which far more resemble the photograph, are carefully made sketches; they were done with the 6-foot.

A vote of thanks was accorded to Mr. Roberts for his paper.

Mr. Maunder. My paper relates to the history of the great group of sun-spots which attracted so much attention in February; and I have been looking through photographs we have at Greenwich to trace it as far back as possible. I found that February was not its first appearance. It was first seen on 1891 November 15, and it can be traced through five successive revolutions of the Sun (in November, December, January, February, and March). There are two or three points about the group which make it a remarkable one, quite irrespective of the great size it attained in February. In November 1891 the group was in south latitude 18°; during December its latitude was 20°; during January 1892 25°, and during February 28°; the same latitude being maintained in March. There was therefore a steady motion in latitude away from the equator from November 1891 to February 1892, but that motion ceased after the spot had attained its greatest size in February. The spot group did not reappear after passing round the west limb on March 17, but a small spot was seen in the same longitude, but much nearer the equator, during the early days of April.

During November the great spot-group was attended by two other groups at a little distance from it; they were in fact so near to it that the great group of February would have included all three groups in its area. These two groups were perfectly distinct from the main group, one lying to the north of it in south latitude 11°, but having the same longitude, and the other preceding it by 20° of longitude, but lying in the same latitude. The group which was nearest the equator in November appeared to be the one seen in the early days of April. During the later days of April and in the early days of May we again had a large group of spots on the Sun in practically the same place as the great group occupied during January, that is, in longitude about 255° and south latitude 24°. There are many interesting details about the behaviour of the main group and the two attendant groups, the chief one being the intermittent way in which some of the spot groups appeared and disappeared.

Looking backwards, I find that on October 25, 1891, a small group of spots was seen about the place of the great spot, and that on September 27, 1891, there was a tiny spot seen for a single day in the same latitude and longitude as the great spot-group was two months later. We have, therefore, an area on the Sun which has been more or less disturbed for nine successive rotations of the Sun, and during five of these rotations the spot-group showed the remarkable drift away from the equator to which I have alluded.

Mr. Turner. We have an important paper from *Mr. Stone* on his observation of the Moon at the Radcliffe Observatory, and several papers on the new star, as well as others on the eclipse of the Moon, 1892, May 11, with observations of occultations of stars, and one by Mr. Newall on a useful spectroscopic diagram; but time will not allow them to be read.

The following papers were announced:—

R. T. A. Innes. "Note on the Secular Perturbations of the Earth by Mars."

E. B. Powell. "Note on the Orbit of α Centauri."

J. E. Gore. "On the Orbit of γ Centauri."

H. C. Russell. "Photographs of Comet Swift, March 1892."

J. E. Gore. "On the Orbit of γ Coronæ Australis."

E. S. Holden and *W. W. Campbell.* "Negatives of Jupiter made with the Great Telescope of the Lick Observatory during 1891."

Rev. A. Freeman. "On the Illumination of Saturn's Rings during Sunrise, and on recent Observations of their Reappearance."

Isaac Roberts. "Photographs of the Region of the Crab Nebula."

Isaac Roberts. "Photographic Search for a Planet beyond the Orbit of Neptune."

F. Folie. "On the Formulæ of Reduction to apparent places of close Polar Stars."

Rev. W. Sidgreaves. "Spectrum of Nova Aurigæ."

Royal Observatory, Greenwich. "Observations of Comet *a* 1892 (Swift)."

W. H. M. Christie. "On a new Form of Altazimuth."

E. J. Stone. "Observations of the Moon made at the Radcliffe Observatory, Oxford, during the year 1891, and a Comparison of the Results with the Tabular Places from Hansen's Lunar Tables."

Radcliffe Observatory, Oxford. "Observations of Occultations of Stars by the Eclipsed Moon on 1892 May 11."

Royal Observatory, Greenwich. "Partial Eclipse of the Moon, 1892 May 11."

J. G. Lohse. "Estimations of the Magnitude of Nova Aurigæ."

H. F. Newall. "On a Diagram useful as a Guide in adjusting a Diffraction-grating Spectroscope."

H. F. Newall. "Comparison of Magnitudes of Nova Aurigæ and neighbouring Stars, made with the Newall Telescope, Cambridge Observatory."

A. Marth. "Ephemeris for Physical Observations of Jupiter."

Rev. W. H. Addison, 8 West Chapel Street, Mayfair, W.; *Lieut. Henry Baynham, R.N.,* H.M.S. 'Wellesley' Training-ship; *Humphrey Barker Chamberlin,* 1033 Sixteenth Street, Denver, Colorado, U.S.A.; *Otto Jaffe,* Kin Edar, Strandtown, Belfast; *Charles Henry Johns,* M.A., Althorpe House, Waverley Grove, Hendon, N.W.; *William Grant MacGregor,* 18 Coleman Street, E.C.; *Capt. R. Reynolds,* Lieut. R.N.R., U.S.S. 'Pretoria,' Southampton; *Albert Edward Watson, B.A.,* Whitgift Grammar School, Croydon, were duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

Rev. J. B. Brearley, M.A., St. James' Church, Oldham, Lancashire (proposed by A. de Boë); *Herbert Hancock, M.A.,* Mathematical Master, Bancroft's School, Essex (proposed by W. H. Besant); *Thomas Torreno Knowles, M.A.,* Mathematical and Science Master, Royal Grammar School, Lancaster (proposed by the Rev. J. Bone); *Alfred Thomas Odell-Lorrell,* Science Lecturer and Examiner, 1 St. Mary's Grove, Chiswick; *Edward Stroud,* Prisca Coborn Foundation School, Tredegar Square, London (proposed by J. McCarthy); *John Tatlock, Jun., M.A.,* New York, U.S.A. (proposed by S. W. Burnham).

As Associates, proposed by the Council:—

W. L. Elkin, Yale College Observatory, New Haven, Conn., U.S.A.; *J. C. Kapteyn,* Observatory, Groningen, Holland; *Hugo Seeliger,* Königliche Sternwarte, Munich; *Hermann Struve,* Observatoire, Pulkowa, Russia.

BRITISH ASTRONOMICAL ASSOCIATION.

The sixth ordinary Meeting of the second session was held in Barnard's Inn Hall on Wednesday, April 27, Capt. Noble, the President, in the chair. Sixteen new members were elected, and the names of twelve candidates for election were read and suspended.

Among the presents received were £5, the balance from a course of lectures on astronomy which Mr. Schooling had been giving, and 120 negatives presented by Mr. Ledger for the use of the photographic section.

Mr. Vallance described a model of a convenient and cheap form of altazimuth stand designed and used by himself. Two telescopes, a refractor and a reflector, were mounted on the same stand, and so arranged that the eyepieces of both were in the centre of motion. The telescopes were raised by their own weight and pulled in one direction in azimuth by a cord attached to a weight. The reverse motions were obtained, and these controlled by cords fixed to small winding apparatus.

The President, having commented on Mr. Vallance's stand, asked Mr. Brashear to give an account of some of the instruments which he had recently manufactured.

Mr. Brashear presented the Association with photographs and full drawings of the new spectroscope for Prof. Young, of Halstead Observatory, and also of the new photographic instrument for the Lick Observatory. He had recently constructed new spectroscopes for the U.S. Naval Observatory and for Prof. Young, the latter being one of the best ever yet made. New photographic apparatus had been supplied to the Allegheny Observatory, to Prof. Hale, and to the Lick Observatory. Better photographs had been taken with this last than had been before obtained. He had heard just before leaving America that it had been proposed to build a free observatory in addition to Mr. Phipps's conservatory.

The President having thanked Mr. Brashear for his address, Mr. Adams read a paper upon the "Apparent Connection between Sun-Spots and Magnetic Disturbances." He pointed out that the westerly position, suddenness of inception, and short duration of the magnetic effects of Sun-spots were incomparable with superficial (photospheric) production, and to explain these phenomena propounded a theory that the Sun consisted of a solid nucleus within a viscid envelope, the former revolving at a much higher rate than the latter, and thus acquiring magnetic properties.

The President said that he should think that the viscid exterior would act as a brake to the nucleus, and both would after a time take up the same rate of revolution.

Whilst the ballot was proceeding, Mr. Maunder read a communication which had just been received from Miss Brown, to the effect that she had observed a rose-coloured flush over the whole surface of the Sun at 4 P.M. on April 26th. This phenomenon lasted a few minutes and then faded.

Mr. Sadler called attention to the fact that Secchi and others had noticed red flushes over Sun-spots.

Mr. Hardy suggested that it might have some connection with the aurora visible that night.

Mr. Sadler gave notice that Jupiter would occult a small star on May 24th, at $3^h\ 35\frac{1}{2}^m$ A.M.

Capt. Noble mentioned that he had heard that one of the New South Wales observers had seen a star occulted by Jupiter, and it appeared actually to invade the planet. He stated that a letter had been received from Mr. Denning suggesting that the *précis* of current astronomical publications should be left out of the journal, and be replaced by papers of especial interest to amateurs.

After some discussion it was unanimously agreed not to alter the journal.

The meeting was then adjourned until May 25th.

ROYAL METEOROLOGICAL SOCIETY.

THE usual monthly Meeting of this Society was held on Wednesday evening, May 18th, at the Institution of Civil Engineers, 25 Great George Street, S.W., Dr. C. Theodore Williams, M.A., President, in the Chair.

Mr. B. E. C. Chambers, Mr. R. Law, F.C.S., Dr. W. A. Sturge, and Dr. E. Symes Thompson were elected Fellows of the Society. Five Honorary Members were also elected, viz.: Mons. A. D'Abbadie, Dr. W. H. von Bezold, Dr. R. Billwiller, Mons. N. Ekholm, and Prof. P. Tacchini.

The following papers were read:—

"Raindrops," by Mr. E. J. Lowe, F.R.S., F.R.Met.Soc. The author has made over three hundred sketches of raindrops, and has gathered some interesting facts respecting their variation in size, form, and distribution. Sheets of slate in a book form, which could be instantly closed, were employed; these were ruled in inch squares, and after exposure the drops were copied on sheets of paper ruled like the slates. Some drops produce a wet circular spot, whilst others, falling with greater force, have splashes around the drops. The same sized drop varies considerably in the amount of water it contains. The size of the drop ranges from an almost invisible point to that of at least 2 inches in diameter. Occasionally large drops fall, that must be more or less hollow, as they fail to wet the whole surface enclosed within the drop. Besides the ordinary raindrops, the author exhibited diagrams showing the drops produced by a mist floating along the ground, and also the manner in which snow-flakes, on melting, wet the slates.

"Results of a Comparison of Richard's Anémo-Cinémographe with the Standard Beckley Anemograph at the Kew Observatory," by Mr. G. M. Whipple, B.Sc., F.R. Met.Soc. This instrument is a windmill vane anemometer and is formed by six small wings or vanes of aluminium, 4 inches in diameter, inclined at 45°, rivetted on very light steel arms, the diameter of which is so calculated that the vane should make exactly one turn for a metre of wind. Its running is always verified by means of a whirling frame fitted up in an experimental room, where the air is absolutely calm, and, if necessary, a table of corrections is supplied. The recording part of the apparatus differs entirely from any other anemometer, and is called the Anémo-Cinémographe, and in principal is as follows:—The pen, recording on a movable paper, is wound up at a constant rate by means of a conical pendulum acting as a train of wheel links, whilst a second train, driven by the fan, is always tending to force it down to the lower edge of the paper; its position, therefore, is governed by the relative difference in the velocity of the two trains of wheel-work, being at zero when the air is calm, but at other times it records the rate of the fan in metres per second. The author has made a comparison of this instrument with the Standard Anemometer at the Kew Observatory, and finds that it gives exceedingly good results.

"Levels of the River Vaal at Kimberley, South Africa, with remarks on the Rainfall of the Watershed," by Mr. W. B. Tripp, M.Inst.C.E., F.R.Met.Soc. Measurements of the height of the River Vaal have for several years past been made at the Kimberley Waterworks. These gaugings having been placed at the disposal of the Society the author has compared them with the rainfall of the watershed. There is a marked period of floods and fluctuations at a comparatively high level from about the end of October to the latter part of April, and a period of quiescence, during which the river steadily falls, with very slight fluctuations, from about April 19th to October 31. The highest flood, 52·5 feet, occurred in 1880; the next highest being 50·3 feet on January 24th, 1891.

ASTRONOMICAL SOCIETY OF THE PACIFIC.

The Annual Meeting of this Society was held at San Francisco, March 26th, 1892.

President Pierson presided.

The Secretary read a list of presents received since the last meeting, and the thanks of the Society were voted to the givers.

Thirty-two new candidates were duly elected.

The membership of the Society is now 432, of whom 51 are Life Members.

The Treasurer reported:

Receipts for the year	$3,397
Expenditures „	2,490
Cash in Banks	3,841

The Committee on Comet Medal reported that medals had been awarded to Mr. E. E. Barnard, for the discovery of Comets *a* and *e* of 1891.

The medal is awarded only for unexpected Comets.

The following papers were presented:—

1. "Annual Address," by the retiring President, Hon. Wm. M. Pierson, San Francisco.

2. "The Sun's Motion in Space," by W. H. S. Monck, Dublin.

3. "How to find Celestial Objects with an Equatorial Telescope without the Aid of a Sidereal Time-piece," by Chas. Burckhalter, Oakland.

4. "Astronomical Observations in 1891," by Torvald Köhl, Odder, Denmark.

5. "The Harvard College Observatory Astronomical Expedition to Peru," by Mrs. M. Fleming, Cambridge.

6. "Discovery of New Rills on the Moon on Lick Observatory Negatives," by Prof. L. Weinek, Prague.

The following officers of the Society were elected to serve for the ensuing year:—

President: J. M. Schaeberle.
Vice-Presidents: E. J. Molera, Frank Soulé, O. v. Geldern.
Secretary: W. W. Campbell.
Secretary & Treasurer: F. R. Ziel.

The meeting then adjourned to meet at the Lick Observatory, June 11th.

F. R. Ziel, *Secretary.*

Are there at present Active Volcanos upon the Moon?

It has been said that had an astronomer been placed upon the Moon at the beginning of the Christian era, and been furnished with our present optical facilities, that it is very doubtful if he would have detected any of the terrestrial volcanic eruptions that have occurred since that date.

Before describing at length the present researches, it may be well to say a word or two about the atmospheric conditions which are found at our mountain observatory. The steadiness of the air here is so marked, that with our 13-inch Clark refractor it has not been found necessary for any of our work, saving such as required a large field, to use an eyepiece of lower power than two-fifths of an inch, giving a magnification of 475 times. With this power definition is frequently obtained which compares favourably with that found in our 8-inch finder with a power of 60 diameters. This was the highest power that we possessed when these researches were begun, but other eyepieces have since been received, and it has been found that powers of 800–1200 can usually be used here to advantage.

With such definition, one of the first objects upon which we turned our telescope was the Moon. Since Schmidt's announcement of a suspected change in Linné in 1866, this crater has received more or less attention from the astronomical world. If this change were genuine, and due to volcanic agency, it would certainly seem that one of the most promising places in which to look for a similar event would be in the surrounding regions of the Mare Serenitatis. On this plane there exist numerous small crater-cones, which, if less conspicuous than Linné is said formerly to have been, are at least equally well authenticated. A careful survey of this region was accordingly undertaken with our 8-inch finder and a power of 60 diameters. This low power was chosen purposely, as giving results more nearly comparable with those of earlier selenographers. A comparison of our observations was then made with the chart published by Neison in 'The Moon.' Of a total of 67 craters, 32 were found common to both charts, 24 were given by Neison and not found by myself, while 11 were found by myself that were not given by Neison. This region was then re-examined with the 13-inch, using a power of 160 diameters. All of the craters mentioned by Neison were then found save two, located near Bessel. Some of the others were not quite correctly placed upon the map, but they were in all cases so nearly right as to leave no doubt as to the crater it was intended to represent. The eleven other craters found by myself were all confirmed, and a great number of other smaller ones were also seen. Three of these craters, one located to the south of Bessel and two others near the southern border of the Mare, were so large and conspicuous that they could be readily seen with a very small telescope. On investigation, they were found also upon a photograph taken by Rutherfurd in 1871, so that it is probable that they were merely overlooked in the construction of the map, and certain that they have existed for at least twenty years. In the absence of evidence to the contrary, the same assumption must be made in regard to the remaining eight craters.

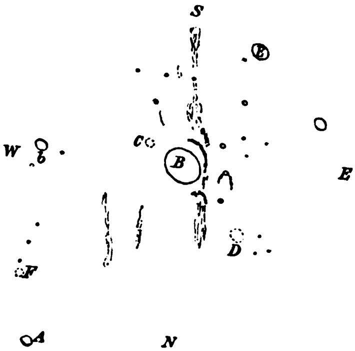
Of the two craters given by Neison which it was impossible to find, even with a power of 800 diameters, one, which we will call D, is situated just to the north-east of Bessel upon Map III. A central crater is drawn, and the outer slopes indicated by hatchings, which would imply a fairly conspicuous object. The other missing crater is less conspicuous upon the map, but is designated by Neison as Bessel C. He says, p. 193, "From Bessel extend several mountain-ridges, and close south-east [south-west?] is a small crater-pit C, in +21° 26′ lat. and +18° 0′ long.; a smaller one lies between A and *b* but nearer A, whilst further off, south-east and north-east, are two others." The last phrase is not very clear—there are two craters north-east and south-east of Bessel, which are further from it than is crater-pit C, according to the map. One of these is represented as a small hill, with hatchings, between Bessel and D; the other is not shown, but may perhaps explain the confusion in his reference to C as being south-east of Bessel, whereas, according to the map and also by the latitude and longitude given, it should be south-west.

A crater-pit lying between A and *b*, and nearly in the position indicated by Neison, was found without difficulty, but it is very minute, and is decidedly smaller than the present crater-pit of Linné, which is perhaps 2500 feet in diameter, and is usually considered to be a rather difficult object. It seems somewhat singular that this minute and difficult crater-pit should have been picked out for reference by Neison, when there are a number of other larger and more conspicuous ones in the immediate vicinity, including two others also lying between A and *b*, but nearer *b*, one of which is nearly as large as the crater of Linné itself. Must we consider that the whole of this portion of Neison's statement, as well as the map, is confused and erroneous, or have we evidence here of the diminution in size of one crater and the total disappearance of two others? In regard to crater C, which was apparently observed with sufficient accuracy to determine its latitude and longitude, it should be said that in its place is now found a faint whitish spot, resembling in appearance that surrounding Linné, but much less conspicuous.

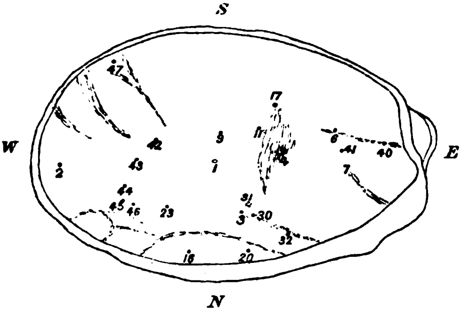
Another region that has often been suspected of change, or at least of volcanic activity, is that surrounding Aristarchus. Certain it is that in the immediate vicinity of this crater and of Herodotus, when the definition is very fine, there are found a number of minute craters that have the appearance of giving out small clouds of steam. Such appearances, however, are deceptive, and may possibly be due to white stains upon the rocks, or to some other unsuspected cause. The most satisfactory evidence of volcanic activity must always be some permanent change which has been effected by it, and this, judging by terrestrial analogies, will also be the rarest.

Some twenty years ago a report upon the phenomena exhibited by the floor of Plato was communicated to the British Association. This crater was therefore selected for careful study with the large refractor, and was viewed under the best obtainable conditions. The Report of the Committee mentions 36 luminous points, four of which—nos. 1, 3, 4, and 17—were generally visible, and were easily recognized as crater-cones. In six others, nos. 7, 9, 11, 30, 31, and 32, the central crater was also visible; while six more, nos. 5, 13, 14, 16, 19, and 22, probably belonged to the same class. For most of this information I am indebted to Neison's work on the Moon, as I have not the Report of the Committee at hand. In the first of these three classes crater no. 4 seems to have now entirely disappeared, nothing but a whitish stain at present occupying its place. In the second class nos. 7 and 31 are missing. The latter of these seems the more singular, as it occupies a place very readily found, being near nos. 3 and 30, and according to a photograph which I possess of the map published by the Committee, 31 should be more conspicuous than 30. Craters 4 and 31 were both looked for with the utmost care, but without success. With regard to 7 the case stands somewhat differently. There is at present certainly no crater in the place indicated by the Committee; but a large crater, perhaps the fifth in order of size, is found about eight miles south-east of the position of 7, as indicated by them. It does not seem likely that so glaring an error should have been made in its location, nor does it, on the other hand, seem probable that so large a crater should have been overlooked by them.

In the third class no. 16 was the only crater observed, the remaining five being missing. Of the remaining twenty luminous points four were recognized, namely nos. 2, 6, 20, and 23 (?). Of these, the latter is a little uncertain, as its position does not exactly agree with that given by the Committee. All of these were found to have central craters. Besides these, eight other craters were detected, not shown upon the map of the Committee. One of these was nearly as large as 31, while two were of the same size as no. 11 in the second class, which was estimated at 1000 feet in diameter. The other five were smaller. Little stress is laid upon the lack of corroboration of the craters of the third class or of the twenty luminous points, because all of these objects must have been very near the limit of vision of the members of the Committee, with the means they had at hand, and they themselves expressly state that they were variable, although this last statement seems almost impossible



Region around Bessel. Scale 32 miles to the inch.—B. Bessel. E. Large crater omitted by Neison. C, D, F. Craters drawn and described by Neison, which cannot now be found.



Plato. Scale 16 miles to the inch. 4, 7, and 31. Large craters drawn and described by a Committee of the British Association, but which cannot now be found. Numerous small craters described by the Committee have been omitted for this same reason. 40, 41, 42, 43, 44, 45, 46, and 47. Craters omitted by the Committee.—Estimated sizes of the various craters in feet. Where several craters are nearly of the same size, they are named in the order of their visibility. 1, 4000. 3, 17, 3000. 31, 2500. 40, 2000. 9, 11, 41, 2, 47, 1000. 32, 20, 16, 6, 900. 42, 800. 43, 44, 46, 600. 45, 500. The sizes of the last-named craters are necessarily very uncertain, but they probably do not exceed the dimensions given.

of explanation. It does not seem likely, on the other hand, that they could have been mistaken with regard to the position of craters where even the central depression was recognizable.

Besides the thirty-six bright craterlets or spots, the Committee report twenty-seven bright streaks, which latter they suggest seem to be intimately connected with the former, both being more or less liable to variations in intensity or visibility. Of these twenty-seven bright streaks, seven can now be more or less clearly distinguished. It is possible that further search under varying illuminations might reveal others, as less time was spent upon this matter than upon the crater-cones.

Summarizing this article, I should like to particularly direct the attention of those interested in such matters to the question of the former existence of the three craters delineated by Neison, in the immediate vicinity of Bessel, and to the former existence of craters nos. 4, 7, and 31 of the Committee upon Plato. Should an actual change caused by the real disappearance of any of these objects be generally conceded, as such, I think the question may well be raised whether volcanic activity upon the Moon is really dead. That all the walls of Linné should have fallen in at the same time, through the mere action of sunlight, without appreciable aid from atmosphere or frost, seems singular, if true; but now that we are beginning to be able to study the smaller lunar craters to advantage, and so many changes are noted, it does not seem as if the same cause can have affected so many of them in the same way, nor does it seem as if all the changes noted can be due to erroneous delineation.

Arequipa, Peru. WILLIAM H. PICKERING.
1892, March 28.

Notes on the Lick Observatory Lunar Photographs.

IN the examination of two lunar photographs taken at the Lick Observatory on Aug. 16 and 27, 1888, I have found several features of considerable interest which do not appear in the standard lunar maps. In the first of these photographs the west wall of Copernicus is on the morning terminator; in the second, the east wall of Julius Cæsar is on the evening terminator. I also examined the Moon on the evening of May 7, 1892, with an 8-inch Clark refractor (of the Chabot Observatory, Oakland, Cal.) and a power of 300+; the terminator being in about 45° east long. The definition was quite good, but not extraordinarily so. I beg to present the result of my observations.

The anonymous walled-plain of the interior of which Hell is the most prominent feature is shown very well in the photograph of Aug. 16, which shows that Neison misplaces the craters Hell C and *e*, putting them too near Lexell, while Schmidt places them correctly. Neison draws a "very distinct" central peak in Hell

C which is not shown by Schmidt, nor could I see it with the 8-inch, yet the photograph shows a low peak in the west central part of Hell B which Neison does not show. The photograph shows a confluent crater-row running from a point (on the line joining the centre of Hell B and Walter *a*) halfway between centre of Hell B and foot of inner slope of Walter's wall, to the N.N.E. side of Lexell. Neison does not show this formation, but Schmidt does in an approximate way. That is, he does not represent the crater-row as confluent nor even as continuous throughout its entire length. With the 8-inch, the Moon's phase was too far advanced to allow this row to be at all well seen; nevertheless the confluent character of the southern or more distinct portion was obvious. About midway in its length a deep crater was easily seen which did not seem to have any physical connection with the row, the latter appearing to skirt it on the east. This hardly agrees with the photograph, which, by the way, does not show the deep crater. The valley which Neison draws crossing the walled-plain from S.W. to N.E. is plainly indicated in the photograph, and instead of terminating at the N.E. wall it crosses it and ploughs a furrow through the crest of a mountain-range nearly to the N.W. wall of Pitatus.

This photograph shows a crater in the S.E. inner wall of Lexell that I have not been able to identify with certainty in the maps of either Neison or Schmidt. For the benefit of those who will wish to identify it with the telescope, I will say that it is halfway between the gap in the wall (Neison's β) and the south end of the valley in the wall.

In this photograph the ridge, part of which lies between Pico B and *e*, is particularly well shown; at the northern end is a triangular black shadow which indicates the presence of a steep peak. Neither Neison nor Schmidt show this peak, though Schmidt draws a little crater just beyond the north end of the ridge.

Neison (Map XIV.) draws an elongated crater or valley between Ball and Sasserides C, which corresponds accurately to the photographic representation of it given in the photograph of Aug. 16. Schmidt draws a double crater in the same place, the smaller one being to the north. This corresponds pretty well to the photograph of Aug. 27, except that in the photograph the southern component of Schmidt's double crater does not look like a true crater, but like a mere depression between hills. An interesting feature in connection with this formation is shown in the photograph of Aug. 16, in the shape of a furrow running down the south wall of Ball to its floor. Observations of this with a 16- or 20-inch telescope and a power of 1000 (provided the seeing be "perfect") are particularly requested.

The photograph of Aug. 27 shows what appears to be a crater (if I mistake not) adjoining Purbach A on the S.E. Neither Neison nor Schmidt draw any such crater. Schmidt shows some ridges south of Purbach A, only one of which adjoins it, and that on the S.W.

It may be interesting to note that I find a 2-inch magnifier very useful in scrutinizing the L.O. lunar photographs, and in some special cases a 1-inch, in spite of the fact that the photographs lose their sharpness under magnification. For instance, under the magnifying-glass, the photograph of Aug. 27 shows very well the two craters Neison draws south of Piton, yet these are utterly invisible to the naked eye even at a distance of 6 inches. A long and careful comparison of the L.O. photographs with the Moon as seen in telescopes has convinced me that they show nearly, if not quite, as much detail as a 3-inch telescope in good weather. I must be understood as referring only to refractors. As for reflectors, a drawing of "Jannsen and Neighbourhood" made with an 8½-inch reflector on a good night appeared in the 'English Mechanic' for April 22, 1892, which shows but little more detail (if we confine ourselves to what was seen with certainty) than a L.O. photograph (moon's age, 5 days) with which I have compared it, while the photographic representation of Fabricius is decidedly superior.

Neison's statement (page 295) that the plain between Copernicus and Eratosthenes appears perfectly free from craters thirty-six hours after sunrise was found (with the 8-inch refractor) to be grossly inaccurate. A large number of crater-pits, some of them forming crater-rows, were visible in this plain at a glance.

With the 8-inch, considerable attention was paid to Clavius. The peak which Schmidt draws in Clavius *e* was distinctly seen, though rather difficult. Clavius *t* was a mass of detail over which I might have well spent all the time at my disposal. In place of the double crater-pit that Neison maps to the east and a little north of Clavius *t*, was a single large crater-pit. (This I saw once before with a 3¼-inch refractor, Copernicus being on the morning terminator.) Perched on the S.W. wall of Clavius *i*, I distinctly saw a craterlet. Continuing the line drawn through the centre of *i* and the crater on its wall (produced it would pass between *a* and *t*), it intersects a crater-pit distant from the wall by the diameter of *i*. Producing this line in the opposite direction to a point half a diameter from *i*'s wall, a little S.E. of this point is a crater-pit. Neison draws two ridges abutting against Clavius *b* on the east. In the angle which the northern one forms with the wall of Clavius *b* is a prominent crater. Distant from this crater by about half its diameter, and right on the crest of the northern ridge, I saw a very minute crater.

With the 8-inch, I saw under vertical illumination a very distinct white ring, indicating the presence of a crater about the size, perhaps, of Proclus *c*, in the approximate position W. long. 38° 20′, N. lat. 12° 10′, following Neison's Map I.; it is the most distinct formation of the kind in its own immediate neighbourhood and is not shown by Neison, neither can I identify it on Schmidt's map.

ROGER SPRAGUE.

Berkeley, California, 1892, May 10.

Selenographical Notes.

Bailly.—There are many points of resemblance between this immense enclosure and Schickard, its not distant neighbour on the north. Both are characterized by very irregular borders, broken in places by ring-plains, large and small, surrounding vast plains which are diversified by a multitude of ringed depressions, ridges, isolated hills, and other features. The area of Bailly is more than 17,000 miles, being about one-twelfth greater than that of Clavius, the only named formation, not classed as a "Sea," which approaches it closely in size. Despite the assertions of some selenographers, its individuality is fully as strongly marked as that of Schickard, if it is observed at a suitable phase of libration and illumination, though owing to its closer propinquity to the limb, its details are never so well made out. In one respect, however, it differs from this object. The west border of Schickard is very broad, especially the southern section of it, while that of Bailly, north of the ring-plains *a* and *b*, is extremely attenuated and uniform in width. About midway, a prominent little ring-plain breaks its continuity, and still further towards the north a crater-chain, on the floor, close to its inner foot, gives it the appearance of being wider than it really is.

The opposite or eastern wall is so closely associated with the flanks of the Dörfel range that one is apt to regard the latter as forming part of it. As sunrise gradually brings these towering heights into greater prominence, it is, however, apparent that valleys intervene which, if viewed under a less oblique angle, would appear to be of considerable width. The inner slope of this wall displays near its southern end four or five very curious parallel valleys, slightly curved, running from its crest to the floor, and in other parts many terraces, all very manifest when the summit of the border is on the terminator.

The two most noteworthy ring-plains in connection with Bailly figure on the south-western side of the formation and are marked *a* and *b* in Neison's Map XVI. They are conspicuous and beautiful objects at the phase just mentioned. The larger and more northerly, *b*, is about double the size of its companion *a*, which partially encroaches upon it. Schmidt alone shows the little central mountain in the latter, though I have seen it several times, notably at 9^h on February 11th, this year, when its presence was indicated by a very distinct notch in the otherwise even contour of the shadow of the west wall of *a*.

However favourable the conditions of libration may happen to be, the interior of Bailly is so foreshortened, that the circular enclosures, of which there are many, are presented as such very elongated ellipses, that it often requires much attention and good definition to distinguish their true nature. Two especially, under the north-east wall, and a third, near the centre of the floor, may easily be taken for bright ridges. Of these features there is an abundance, almost all of them ranging longitudinally. One, running from the south side of the more southerly of the two ring-plains just mentioned to the south-east flank of the ring-plain *a*, extends in an unbroken line for a distance of at least 150 miles. Its shadow on February 11th was a sharp narrow black line, having all the appearance of a cleft. On this evening I noticed that it was crossed at right angles by an undoubted cleft (apparently unrecorded and not shown in any map known to me), extending from the east wall of *b* to the foot of the east wall of Bailly.

No formation of its class presents a more beautiful telescopic picture when on the terminator than Bailly, if it be observed when the foreshortening is at its minimum. This is mainly due to the contrast between the tone of the floor and that of the region beyond the border, and to the great variety of detail included by the circumvallation. Like the interior of Schickard, the tint of the floor is very uneven. At the northern end three or four fan-shaped light streaks, on a dark area, traverse it from east to west.

Webb draws a large irregular enclosure (417) north-west of Bailly, which is named Rosse in his map. It consists merely of a concourse of ridges and does not deserve a distinct appellation. Neison transferred the name to a much more worthy object,—a conspicuous crater in the Mare Nectaris north of Fracastorius.

Schröter gives three large drawings of Bailly, which are among his best efforts and show all the prominent features on the floor.

Fra Mauro B.—I must still maintain that Neison has not misplaced this crater in his Map XIII., as Mr. Sprague asserted in his first communication on the subject. It stands in the position indicated in the text, ridges and all. Mr. Sprague said no word about Fra Mauro A and Σ, beyond remarking that the former was an easy object with his spy-glass, nor did he refer either to the "relative positions" of these or any other of the numerous objects in the vicinity. Now, however, on examining the map, after the perusul of his letter in the last number of the 'Observatory,' I find that both A and Σ are set down somewhat too much to the south, and that to this extent Mr. Sprague's criticism is just.

Beaumont House, Shakespeare Road,
Bedford, 1892, May 19.

Thos. Gwyn Elger.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Early Transits of Mercury.

Gentlemen,—

Mr. Prince is in error in believing that the supposed transit of Mercury in 1278 was the first mention of these phenomena. The first dates from the early part of the ninth century, and is thus mentioned in Tycho Brahé's 'Historia Celestis':—"A.D. 807. Aimóinus Monachus etiam annotat hoc anno die 17 Martii maculam in sole visam per octo dies, quam putat fuisse Mercurium. Sed ad Tab. Pruten. Mercurius longè a sole ad hoc tempus abest." The fact that this phenomenon was seen for eight days shows that some large solar spot must have been noticed during two thirds of its visible passage across the Sun's disk. The year 1278 does not seem to have been a transit year. There were, I believe, November transits in 1276 and 1289, and a May transit in 1273. It would not have been an impossibility for a transit of Mercury to be observed even at this early date. The year 1278, the year mentioned by Mr. Prince, was that in which the prince of philosophers, Roger Bacon, was thrown into prison on account of his studies and foresight, and he writes that "lenses and specula may be so figured that one object may be multiplied into many, that those which are situated at a great distance may be made to appear very near, that those which are small may be made to appear very large, and those which are obscure very plain; and we can make stars to appear wherever we will." It is still possible that an unconscious observation of a transit of Mercury may be found previous to that of 1631, as sun-spot observations of a similar character to those given in Scheiner's 'Rosa Ursina' and in Harriot's MS. might indicate the small spot of Mercury on the Sun in 1615, 1618, or 1628.

While on this subject I may remark, has it ever struck anyone how well Mr. Eyre Crow's picture of Horrox's transit of Venus, in the Royal Academy last year, would answer for the transit of Mercury in 1631 as observed by Gassendi? While the spot is too small for that of Venus it would answer for Mercury. Its position, as painted on the Sun's disk, while utterly wrong for Venus, would answer for Mercury in 1631. Horrox represented as returning from work to examine his reflected solar image, might well be taken for Gassendi returning about 9^h A.M. when the Sun broke through the mist in 1631, at the time he found the assistant who was to take the Sun's altitude had deserted his post.

Faithfully yours,
S. J. Johnson.

Melplash Vicarage, Dorset,
1892, May 12.

Transit of Mercury in 1278.

Gentlemen,—

Since my letter to you of the 12th ult. respecting a supposed transit of Mercury in the year 1278, it has been a matter for consideration whether it would be possible to see Mercury on the Sun's disk without the aid of a telescope. The probability therefore seems to be that what Lycosthenes records was merely the appearance of a large sun-spot.

Yours faithfully,
C. Leeson Prince.

The Observatory, Crowborough,
Sussex, 1892, May 5.

The Cause of an Ice Age.

GENTLEMEN,—

In Sir Robert S. Ball's book on 'The Cause of an Ice Age,' he twice gives a formula for the difference between summer and winter that seems quite erroneous. At p. 95, and again in the Appendix, p. 177, he says—"The maximum difference between the lengths of summer and of winter is thus

465 days × eccentricity."

I should reckon it in a way reducible to this, if we call the eccentricity = sin . ϵ

$$\frac{365{\cdot}2422}{\pi}\text{ days} \times (2\epsilon + 2\sin.\epsilon\cos.\epsilon).$$

In p. 95 he obviously makes a minor error, that "with the present eccentricity of the Earth's orbit, the greatest possible difference between summer and winter would amount to 33 days." This doubtless refers to his two diagrams, pp. 100 and 101, and not to the present eccentricity, which admits no more difference than 8 days. On the whole, I fail to see that any colder age than the present could result from the cause he investigates. The real cause of an Ice Age was shown, I think, long before the effects of glaciation had been noted. I refer the change from glacial conditions to the present warmth entirely to Noah's Flood, as explained by Halley in 1694 (Phil. Trans. 1724), through the fall of a comet composed of steam and air. The rain seems to have added in a few days about 440 feet, and a smaller quantity of air raised the barometer some inches. Yours faithfully,

E. L. GARBETT.

26 Claremont Square, N.,
1892, May 10.

The Discovery of Neptune.

GENTLEMEN,—

At this particular time I think the following facts can hardly fail to interest astronomers.

Though my degree was a very modest one, as it approached I read one term with Adams. At that time there was gossip floating in St. John's to the effect that he had been engaged in calculations to discover a planet exterior to Uranus, and had arrived at results. These results were named, and I wrote a letter to a friend describing them. They were wide of the truth, and carried internal evidence of having been pieced together in accordance with Bode's and Kepler's laws. I did not, while reading with him, refer to these reports, from a feeling of delicacy. The portion of the letter communicating them was as follows:—

"St. John's College, Cambridge,
"Dec. 6, 1844.

"MY DEAR ——,

"My tutor, Adams, was the Senior Wrangler of last year but one. There have been some perturbations (or disturbances) of Herschel's orbit, which have puzzled astronomers very much, and Adams attributes them to a *new planet*, yet unseen, beyond the limits of our present solar system; and from scientific registers of these disturbances, he has calculated its mass and orbit on this supposition; and from what I can learn, it is, according to his results, about twice the distance of Herschel from the Sun, and is almost as large as Jupiter, and its periodic time round the Sun is about 250 years. Adams has had these calculations in hand for above a year, and, I hear, has had them in his head ever since he was an undergraduate. He has written, or will write, to Lord Rosse and other astronomers, to direct their observations in the direction in which the body is supposed to be; and we shall hear all about it in due time. Is it not a triumph of mathematical skill when men can calculate, in any degree at all, the orbits of planets they have never seen? I believe the large comet's orbit gave indications of the same nature, which assisted him in his conjectures." [I do not know what comet was meant. The report was that it passed near the supposed place of Neptune, and lurched from its course to an extent correspondent to the main calculations.]

Some years ago my excellent friend Dr. Parkinson, who headed my year, proposed me, from personal knowledge, for a Fellowship of the R. A. S., and told me that Prof. Adams readily added his signature to the nomination. In thanking Adams I told him of the letter; and he asked to see it, and I sent it to him. I also told him that he had given his signature in favour of an old pupil, whom, of course, after some forty years, he had forgotten. The following was his reply:—

"Observatory, Cambridge, 11th Dec., 1884.

"My dear Allen,—

"When I signed your certificate at Dr. Parkinson's request the other day, I did not realize the fact that you were my old pupil. This makes me still more pleased to support your application. Thank you for sending the extract from your observation-book, which is interesting. I cannot recollect having said anything about a comet being disturbed by the unknown planet.

"Believe me, yours very truly,
"J. C. Adams."

I am, Gentlemen,
Yours very faithfully,
Edward Allen.

Castlechurch Vicarage, nr. Stafford,
1892, May 13.

Bayer's Nomenclature.

Gentlemen,—

Mr. Lynn's opinion (vol. viii. p. 379) is that Arago's supposed rule by which Bayer was guided in giving the name α, viz. that he really did endeavour to designate by it the brightest star of a constellation, was only observed by Bayer when one star was very decidedly the brightest. A little investigation and comparison go further than this, and seem to show that Bayer made no difference in his rule between *α* and any other of the Greek letters. Bayer's general scheme was to allot letters to the stars of the same unit order of magnitude in a constellation in alphabetical order according to their position from head to tail (or to feet) of the imaginary figure. The only constellations that give any possible support to Arago's hypothesis about *α* are *Ursa Minor*, *Capricornus*, *Musca*, and *Chamæleon*—the last two new in Bayer's time, and possibly modified since by Lacaille, so that we cannot attach much weight to them. In *Ursa Minor*, *α* and *β* are practically of the same brightness in the Harvard Photometry, and *α* being at the tail the letters should have been reversed according to Bayer's general plan; to my eyes, however, *α* is much brighter than *β*. In *Capricornus*, *α* and *β* are about the same brightness, *α* being only 0·1 the brighter in H.P.; and one cannot see, except for Arago's theory, why the letters should not have been reversed, since *β* is at the root of the horns and *α* higher up. And in *Musca* and *Chamæleon*, *α* is only 0·5 and 0·3 (respectively) brighter than the next brightest star, according to the Uranometra Argentina.

On the other hand, in addition to the two constellations *Hercules* and *Gemini*—besides *Draco*, in which *α* is suspected of variability, and *Cetus*, the probable variability of *α* being discussed in your last No.—quoted in vol. viii. pp. 340 and 376, as exceptions to the rule given by Arago, there are *Libra*, *Sagitta*, *Equuleus*, *Aquarius*, *Orion*, *Cancer*, *Corvus*, *Crater* (where the lettering appears to commence with the stand of the cup), *Delphinus* (in which there seems to be no reason for giving preference to *α*—which is not the brightest star—and *β* over *γ* and *δ*), *Cassiopeia* (in which *α* is the 3 mag. star nearest the head), and *Ursa Major* (where *ε* is somewhat the brightest star, but further off the head than *α*).

Thus, while there are thirteen cases opposed to Arago's theory, there are only four which appear in possible support of it, and in these four cases it may be that *α*, though less than a magnitude brighter than the next brightest star, may nevertheless have been classed in the next unit of magnitude brighter.

In the case of *Gemini* it would appear that Bayer gave the precedence to the right-hand twin as we look at them, unless indeed he paid some attention to the right ascension as suggested by Mr. Lynn (vol. viii. p. 379), the cases of *Libra*, *Delphinus*, and *Capricornus* giving some colour to his supposition. As to *Sagittarius*, the plan of lettering is incomprehensible.

Some of the irregularities in nomenclature may be accounted for if we suppose that Bayer did not employ much care to be perfectly exact in the order of position in which he lettered the stars from head to tail (or to feet); and also that there were inaccuracies in the catalogues from which he derived the magnitudes.

Sunderland, 1892, May 19. T. W. Backhouse.

Comet a, 1892, *Swift*.

GENTLEMEN,—

There has been remarkably little notice of this comet in English periodicals—nothing to give a proper idea of its brightness, or even as to whether it was visible in this country. But through a communication on the subject kindly sent me by Dr. Copeland, I was enabled to have pretty good views of it at the end of last month. I saw it best on the 29th, when at $14^h\ 5^m$ G.M.T. its narrow, straight tail, though faint, was visible to the naked eye to a length of about 11°; the sky there was slightly affected by twilight. On the 30th, at $13^h\ 55^m$, the head was about of the 4th mag., being midway between μ and λ Pegasi in brightness. On the 26th, at $15^h\ 15^m$, the spectrum, as seen with a "miniature" spectroscope on my 4¼-inch refractor, showed the three carbon bands very bright, while there was little, if any, continuous spectrum.

Faithfully yours,

Sunderland, 1892, May 6. T. W. BACKHOUSE.

The Total Solar Eclipse of 1724.

GENTLEMEN,—

It is somewhat remarkable that there is no record, so far as I am aware, of this eclipse, the last which was total in England, having been seen by anyone in this country except Dr. Stukeley, who saw it at Haradon Hill (which, since it was selected as a point in the trigonometrical survey, has been called Beacon Hill), about two miles to the north-east of Amesbury, near Salisbury. His account, which is almost confined to general terrestrial appearances, is given, in the form of a letter to Halley, under Iter VII. of his 'Itinerarium Curiosum.' The date given is May 10, which is an error, for the eclipse took place on May 11 old style (then still observed in England), corresponding to May 22 new style. There was much cloud about, but Stukeley says he did not regret this because "it added so much to the solemnity of the sight." Venus was seen very plainly, but no other star. Dr. Stukeley remarks that he had observed the total eclipse of 1715 from the steeple of Boston Church, Lincolnshire.

We must suppose that the sky was completely cloudy over London (which was just outside the line of totality) and over a great part of England; but I have not met with any statement of this. The map which Halley prepared of the shadow-path is familiar to all Fellows of the R. A. S. Bradley had observed the comet of 1723 in the preceding autumn at Wanstead, but had to go to Oxford in November; he speaks of the deficiency of means of making astronomical observations there, which he hopes will at some future time be provided "in hac illustrissima scientiarum sede," but unquestionably he would have made such observations of the eclipse as were possible had the weather been fine. Shortly after this time "the ingenious George Lynn" commenced his astronomical observations in Northamptonshire; Southwick, where he was located, was out of the line of totality, but unless prevented by clouds, he would surely have recorded something of the phenomenon.

At Paris the eclipse was observed by Maraldi, Cassini, and the brothers Delisle. The duration of totality was 2 minutes 17 seconds, from $6^h 48^m$ to $6^h 50^m$ in the evening. Notwithstanding scattered clouds, Mercury was distinctly seen between the Sun and Venus, besides several fixed stars, including Capella. Maraldi remarks:—"Durant l'obscurité totale, nous n'avons pû distinguer avec la Lunette aucune lumière sur le disque visible de la Lune, mais on voyoit tout autour cette lumière qu'on a remarqué autrefois dans de semblables Eclipses. Elle paroissoit non seulement à la vue, mais encore avec la Lunette, quoiqu'il y eut autour du Soleil des nuages rares. Au commencement de l'obscurité totale, la lumière nous a paru plus grande du côté de l'Orient que du côté d'Occident: au contraire, vers le fin de l'obscurité totale, elle a paru plus grande vers l'Occident qu'elle n'étoit vers l'Orient." Similar changes, as I remarked in Vol. ix. p. 354, of the 'Observatory,' first led Halley, in the case of the total eclipse of 1715, to suspect that the appendages seen on those occasions round the Sun appertained to him and not to the Moon.

Yours faithfully,

Blackheath, 1892, April 4. W. T. Lynn.

The alleged Red Colour of Sirius in Ancient Times.

Gentlemen,—

I have read with much interest the papers in 'Astronomy and Astro-Physics' in which Mr. See has elaborately and with great care examined this question. Though I scarcely think it admits of so positive a settlement as he seems to do, yet he has certainly shown the untenability of the objections which Schjellerup (as I pointed out in the tenth volume of the 'Observatory') brought forward against the alleged change of colour in Sirius.

For it appears probable that the Arabian astronomers spoke of five (Ptolemy's own enumeration) *nebulous*, not *reddish*, stars in the 'Almagest,' and this renders it very doubtful (to say the least) whether the epithet *ὑπόκιῤῥος* was wanting in their copies of that work as applied to Sirius. My remark on the peculiarity of the expression in Ptolemy was founded on this, that after *καλούμενος* Κύων *καί*...., one would have expected a substantive rather than an adjective to follow. But I was quoting from Halma's edition of the 'Almagest,' and I find in older editions that the *καί* is non-existent, and that *ὑπόκιῤῥος* stands on the same ground applied to Sirius as to the other stars to which that epithet is affixed by Ptolemy.

But of course the surpassing brightness of Sirius may have led to its being called "fiery," and this may have given the impression of a reddish colour to those who did not examine the star particularly themselves; I say *reddish*, because Festus, speaking of the "rutilæ canes," to the sacrifice of which such special attention is called by Mr. See, says that it means "non procul a rubro colore."

Miss Clerke pertinently quotes the expression of Lord Tennyson, who in the 'Princess' speaks thus of this star:—

> "......As the fiery Sirius alters hue,
> And bickers into red and emerald."

Of all the ancient testimonies with regard to the alleged red colour of Sirius, I should attach most weight to that of Seneca, who really (for there seems no sufficient ground for altering the ordinary reading) appears to have examined the colour and found the redness greater than that of Mars.

Yours faithfully,

Blackheath, 1892, May 20. W. T. Lynn.

OBSERVATORIES.

Paris.—Admiral Mouchez obviously finds that the present is not an easy time for the Director of a large National Observatory: he has many serious claims on his attention. He feels that the moment has arrived for extending the old buildings and perhaps moving away from Paris altogether; he has to start a new spectroscopic department; and there is the work of the Chart of the Heavens, to which he devotes himself unsparingly. His report for 1891 opens with references to the Meeting of the Permanent Committee for the Chart in March, and with the following items of news:—that a photographic annexe is to be built for storing and working at the plates, for which 20,000 fr. have been appropriated; that the large equatorial coudé is now mounted; that M. Deslandres, the head of the new spectroscopic department, has been partially equipping it at his own expense to avoid delay; and that the statue of Arago is to be properly erected in front of the South Gate. The project of moving the observatory is still *in nubibus.*

The meridian work consisted in regular observations of the Sun, Moon, planets, and fundamental stars (with a fine record of 20,000 observations); and in various special investigations on the latitude. No fluctuations in latitude like those found elsewhere seem to appear at Paris; but a systematic difference of about 1″ between the results found by two different instruments is puzzling. It is perhaps due to residual flexure, although the greatest attention has been paid to determinations of flexure by M. Loewy and M. Périgaud; and this hypothesis seems scarcely tenable. It is suggested with some show of reason that the discordance is due to the bad climatic conditions, and is direct evidence for the necessity of moving the observatory to some place where the city ceases from troubling and the atmosphere is more or less at rest.

MM. Loewy and Puiseux have practically finished their work on aberration and published a preliminary reduction. The complete discussion will be published in 1893, after a historical survey of the subject has been prepared, which it is hoped to complete this year.

Numerous other paragraphs bear witness to continuous activity, and not the least that dealing with Mlle. Klumpke's work. We are glad to see that the third part of the great Catalogue is ready for press.

NATAL.—Report for 1890–91, *i. e.* January 1, 1890, to July 1, 1891, received. The zealous assistance of eight ladies is gratefully acknowledged. "The principal work in progress at the Observatory is the comparison of the declinations deduced from observations made at the Observatories in the Northern and Southern Hemispheres through the comparison by Talcott's method of the zenith distance of northern stars and southern circumpolar stars taken both above and below the Pole. During the last year a considerable number of observations have been obtained and reduced.

"The work of observing pairs of equi-zenith distant stars for determining the latitude of the Observatory has been brought to a conclusion. There have been obtained 1022 observations of 35 pairs of stars. These have all been reduced and tabulated, and it only remains to discuss the meridian observations of the fundamental declinations of the stars used. This work has been undertaken, and it is probable it will be brought to a conclusion by the end of the year.

"During the last eighteen months the entire mass of the meridian observations of the Moon which were made at Greenwich during the eleven years 1851 to 1861, have been reduced and compared with the theoretical basis of Hansen's Lunar Tables in the same manner as those for the sixteen years 1862 to 1877 were reduced, compared, and discussed in the 'Memoirs of the Royal Astronomical Society' for the year 1885. These observations are somewhat over 1200 in number, and are to be employed for further investigation into the theoretical basis of the present lunar tables.

"The whole mass of lunar observations made at Greenwich during the forty years 1851–1890 have now been reduced in the same manner, and the results of the discussion of these observations, over 4000 in number, are now being prepared for the press, and will be published in the coming year.

"The comparison of the reduction of the tidal records of the three years, 1884, 1885, and 1886, which have been already made at the Observatory as the preliminary to the formation of proper Tide-tables for predicting the height and times of the Natal Tides, had shown the necessity of obtaining further data by the reduction of the records of further years. For it was found impracticable, from records extending over only three years, to distinguish between the changes due to the effect of the extension of the North Pier and the South Breakwater and the carrying out of the dredging operations, and to those due to changes in the position of the relative orbit of the Moon.

"In addition to the above work, there has been carried on the regular routine work of the Observatory in connection with the system of Colonial Time Signals, and the regular reading, recording, and discussion of the usual Meteorological Observations.

"A great deal of work has now accumulated at the Observatory, awaiting printing and publishing, and the time seems to have arrived for considering the advisability of issuing a volume of Natal Observations."

WASHINGTON.—The volumes of observations for the years 1886 and 1887 have just come to hand. The volume for 1886 contains, first, the regular meridian observations of the Sun, Planets, and Stars (the annual catalogue containing 703 stars); secondly, observations of numerous double stars, of Nova Andromedæ, and of Saturn's rings and satellites; thirdly, micrometer observations with the 9·6-inch equatorial of comets 1885 V., 1886 I., 1886 II., 1886 III., 1886 VII., 1886 IX., and finally the magnetic observations.

The volume for 1887 contains the regular meridian observations (the annual catalogue containing 348 stars); measures of double stars &c. with the 26-inch, and micrometric observations of comets 1886 VII., 1886 VIII., 1887 II., 1887 III., 1887 IV., 1887 V. In addition there is a report on some magnetic observatories in Europe; and the results of meteorological observations from 1883 to 1887.

WOLSINGHAM (Circular No. 32).—The star DM. $+55^\circ$, 1870: $16^h\ 39^m\ 49^s$, $+55^\circ\ 12'$ ('55); 9·2, was found 7·3, 7·7, April 26, 29. Var. Spectrum like Mira.

PUBLICATIONS.

A REPORT UPON SOME OF THE MAGNETIC OBSERVATORIES OF EUROPE. MAGNETIC OBSERVATIONS AT THE UNITED STATES NAVAL OBSERVATORY, 1890. METEOROLOGICAL OBSERVATIONS AND RESULTS AT THE UNITED STATES NAVAL OBSERVATORY, 1883–1887.—These form respectively Appendix 1, Appendix 2, and Appendix 3 of the Washington Observations, 1887. The report on magnetic observatories is by Ensign Marsh, and resolves itself mainly into a description of the magnetic observatory at Pawlowsk (St. Petersburg), which is stated to be the nearest approach to perfection yet attained. Other observatories become only incidentally mentioned, but very complete particulars are given in regard to the construction of the Pawlowsk Observatory, the character of the instruments, and the methods of reduction employed. Speaking of different systems of instruments, it is mentioned that those at Greenwich are on the Kew system; but this is not so, the Greenwich magnetic observatory having been one of those temporarily established more than fifty years ago, under the auspices of various European governments, at widely different parts of the Earth's surface, long before the application of photography to the registration of magnetic variations, which became introduced at Greenwich in the year 1848. Most of these observations were discontinued after a few years, but that at Greenwich became permanent. In regard to reductions, it is remarked that the Germans in determining diurnal inequality do not separate days of magnetic disturbance, but that at Greenwich the method of General Sabine, of rejecting disturbances beyond an arbitrarily selected limit, is employed, whereas at Greenwich no such rejection is or ever has been made. Only days of very extreme disturbance are omitted. All others are, without exception, included. During the last year or two (since 1889), in addition to the ordinary work, diurnal inequalities have been calculated at Greenwich for five selected quiet days in each month on Dr. Wild's plan, for the purpose of giving means of making a comparison of results with those of such observatories as are unable to effect a more complete reduction; but the old system is also maintained, the results on both methods being included in each annual volume. It is further stated that at Greenwich the magnetic plant is all installed in one building. This is not so, the observations of dip and of absolute horizontal force having always been made in a distinct and separate building, in which the necessary instruments were and are permanently installed.

The volume of Washington magnetic observations for the year 1890 is the second annual issue of results of magnetic observations and records taken at Washington, on an extended and complete plan. Hourly values of declination, horizontal force, and vertical force are given for each day throughout the year, and also the usual determinations of absolute values of magnetic elements.

Appendix 3 contains the meteorological observations made at Washington during the five years 1883 to 1887. They are principally eye observations of the usual meteorological elements taken every three hours, night and day, supplemented by a few general tables of results, including mean daily temperature and pressure in each year. The observations are preceded by an introduction giving an account of the various meteorological instruments of the observatory.

Proceedings of Haverford College Observatory.—Mr. Leavenworth publishes in these 'Proceedings' a series of micrometric measures made to determine the parallax of δ Herculis, and from which he deduces a parallax of 0″·050 ± 0″·014. There are also about 220 measures of double stars with a 10-inch Clark, accompanied with a lament that there are so few. We have taken every opportunity of pointing out that the value of double-star measures is not dependent on quantity. We had rather see these few measures of stars requiring attention than ten times the number of uninteresting couples. There are a few observations of Comet Wolf (1891 *b*) and numerous sun-spot details observed with an 8-inch equatorial.

Bulletin de la Société Astronomique de France.—We are glad to receive a prosperous-looking number of the 'Bulletin' for 1891. The society now has more than 500 members, and the 'Bulletin' contains 200 pages, with many illustrations. The papers treat not only of astronomy, but of more or less allied subjects, such as climatology and electric gyroscopes, and terrestrial magnetism. M. Flammarion has obviously worked hard in the good cause.

NOTES.

The Partial Eclipse of the Moon on May 11.—Fine weather favoured this phenomenon in most parts of the British Isles, and a considerable number of observers seem to have devoted themselves to its study.

As regards the contact of the shadow with craters and other markings on the Moon, a cursory study of the Greenwich Observations, as also of the vast mass of observations obtained at the Total Eclipse of Nov. 15 last, appears to show that while each observer obtains fairly consistent results, there is a very large personality (amounting even to 2 minutes of time) between different observers; the interpretation is of course that the boundary of the umbra is not a sharp line, but a zone of graduated shading of very considerable width.

It was found possible at Greenwich to observe a considerable number of occultations of small stars during the eclipse, and in many cases the results obtained by different observers show a satisfactory accordance. Rev. A. Freeman observed at Sittingbourne 3 disappearances and 3 reappearances, and notes that four of the observations were considered very good. J. Gledhill, Esq., of Halifax, observed 4 disappearances and 6 reappearances, and took 12 photographs at different stages of the eclipse.

The umbra throughout was unusually bright for a partial eclipse, and the limb never became invisible. Near the time of maximum eclipse all the features on the eclipsed part were conspicuously visible, and the colouring was very rich and striking; at Greenwich these colours were noted as bluish green near the bright part, then a zone of slaty grey, and the rest of the Moon ruddy.

Rev. A. Freeman describes the sequence of colours as bluish grey near the bright portion, toning down to a warm orange-grey at the part most in shadow; he describes two singular bright curved wedges of the shadow next E. and W. limbs with bases on the boundary of the shadow, and the points of the wedge quite 40° along the limb away from the shadow. The east wedge was the brighter. They were seen both with the naked eye and the telescope. A. C. D. C.

Occultation of 73 Piscium (Mag. 6·4) by Jupiter on May 23.—This star occurs in the list of probable occultations of stars by planets during 1892 which Dr. Berberich published in Ast. Nach. No. 3073, and to which attention was called in the February number of the 'Observatory,' p. 128. Herr Marth again called the attention of astronomers to it in the 'Monthly Notices' of the Royal Astronomical Society for April. At Greenwich the morning was fine and clear, and the phenomenon was observed with the 10-inch companion to the photographic equatorial. The limb of Jupiter was boiling considerably owing to its low altitude, and great accuracy was not possible; but the following were the approximate times of disappearance and reappearance:—

Disappearance.... May 23^{d} 15^{h} 19^{m}
Reappearance .... 15 33 A. C. D. C.

Comet Notes.—Comet Swift continues to be a very conspicuous and interesting object, and doubtless will attract more attention now that it is at a sufficient altitude for observation before midnight.

The following ephemeris is from Edinburgh Circular, No. 26:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	'		h	m	s	°	'
June 4....	0	1	1	38	48	June 24 ..	0	36	47	45	1
8....		9	11	40	14	28 ..		42	19	46	0
12....		16	51	41	34	July 2 ..		47	15	46	56
16....		24	1	42	47	6 ..		51	29	47	47
20....		30	39	43	56	10 ..		55	3	48	33

At the Meeting of the Royal Astronomical Society on May 13 a very beautiful photograph of the comet taken at the Cape was exhibited, and the President described its physical appearance as seen from the Tropics, the tail being traceable through 10 or 12 degrees.

The brightness on June 2 is 0·38, on July 8 0·16.

Ephemeris of Denning's Comet.

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	'		h	m	s	°	'
June 2....	4	27	3	48	31	June 14 ..	4	53	22	45	21
6....		36	21	47	27	18 ..	5	1	20	44	17
10....		45	6	46	24						

The brightness throughout this period is nearly constant at 0·74, that at discovery being unity.

Continuation of the Ephemeris of Winnecke's Comet.

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
June 4 ..	10	47	16	42	57	June 20 ..	9	57	26	39	36
8 ..		39	47	42	24	24 ..		31	19	37	39
12 ..		29	52	41	43	28 ..	8	55	58	34	24
16 ..		16	17	40	51	July 2 ..		10	46	29	2

Continuation of search Ephemeris of Tempel's Comet.

	R.A.			S. Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
June 4 ..	18	19	59	27	48	June 19..	18	9	2	29	51
9 ..		16	43	28	30	24..		4	55	30	32
14 ..		13	0	29	10	29..		1	4	31	13

Dr. Lamp has published a monograph on Brorsen's Comet, which forms Part vii. of the publications of the Kiel Observatory. He finds no distinct indication of a resisting medium, and concludes that if this is the cause of the anomalies in the motion of Encke's Comet, its action is only sensible within a distance from the Sun of 0·5 or thereabouts. A. C. D. C.

SIRIUS AND ALGOL IN ARABIC.—The first being separately called "Esh-Sh'ary" (*Sh'ar*, to know, to distinguish), Sirius and Procyon are together named Sh'ariin, and also "the Two Sisters of Suheyl" (Canopus). Sirius, Procyon, and Canopus were fabled to have been originally companions, but Canopus having descended into the south, Sirius followed across the Milky Way, and Procyon, being left behind, wept until its light was dimmed. Hence the Greater Dog Star is called "El 'Abour," from the word *'Abr*, signifying to cross, and the Lesser "El Ghumissan," from *Ghums*, a thick exudation in the eye, clouding its sight. The first is also called the Yeminian, or Southern, and its fellow the Syrian, or Northern Sh'ary. Sirius has another name, "El Merzoum," apparently from *Ruzm*, thunder and rain, from its indicating the rainy season. It was worshipped by some Arab tribes, whence Allah is termed in the Kurán, "the Lord of Esh-Sh'ary." At the time of the Flight it rose aurorally in Central Arabia on July 13th, O. S. The Arabs say "When Sirius rises (heliacally), the owner of the palm-trees begins to see" (what fruit his trees will bear). Another figurative saying runs:—"His west wind (a withering one) blew violently, and his Sirius set"; meaning his evil fortune prevailed, and his good fortune departed, for the Dog Star sets heliacally at the beginning of the winter.

Algol is the Arabic El Ghoul, an evil spirit, whose name is derived from a root signifying change of form or aspect, and thus applied to a being capable of undergoing metamorphosis. It is obvious that it was in this sense that the name was bestowed on the Demon Star, whose fluctuations in brilliancy did not escape the keen observation of the sons of Ishmael.

THE AURORA OF 1892, APRIL 25.—A very brilliant aurora was seen at Ealing on the night of April 25. During twilight and early evening the northern sky was noticed as unusually bright and a greenish auroral glow was visible. At 21^h 15^m bright streamers began to form in the N.N.E., the green glow increased in intensity and extended to an altitude of about 10°, while fresh streamers extended from this and covered the whole northern sky from Venus in the west to α Lyræ in the N.E. The streamers were slightly convergent and reached an altitude of over 50°, the pole star being distinctly involved. They were noted as of a slight yellowish-green colour changing very rapidly in intensity and finally fading into a very well-marked crimson glow. At 21^h 48^m the outburst was at its first maximum and from that time steadily decreased until 0^h 0^m April 26. At 0^h 0^m the green glow again increased in intensity and reached an altitude of over 10°, while at 0^h 15^m a second outburst, more vivid and beautiful than the first, occurred. The green glow filled the N.W. sky; above this, and extending to an altitude of over 50°, a brilliant red glow was formed and streamers of yellowish green shot upwards into this, changing very rapidly in shape and intensity.

As each streamer faded the red glow in its vicinity was very much intensified, and this was specially noticeable at 0^h 30^m, when very broad streamers (over 10° in width) were formed. Although the green glow was in the N.W., the first outbursts of streamers were in the N.N.E. and spread towards the N.W. This was maintained in the second outburst, and the same order was noticed when the streamers changed in character from narrow to broad. At 0^h 30^m the second outburst was at its maximum, and from that time the aurora diminished in intensity until at 2^h 0^m only a very faint glow was visible in the N.N.W.

DR. HUGGINS lectured at the Royal Institution on Friday, May 13, on the New Star in Auriga; but as this night was unfortunately selected by the Royal Astronomical Society for their evening meeting, many of us were unable to be present. The lecturer is reported to have suggested that the phenomena were in accordance with the theory advanced by Klinkerfues some years ago, and might have been produced by the near approach of two bodies travelling in opposite directions at great velocities, violently distorted by tidal action, which would generate a great quantity of heat.

AMONG the fifteen Candidates nominated by the Council for election to the Royal Society, we do not see the name of anyone connected with Astronomy.

AN excellent article by M. C. Trépied on "La Carte Photographique du Ciel" appears in Nos. 16 and 17 of the 'Revue Générale des Sciences' (1891, August 30 and September 15).

THE British Association will meet this year in Edinburgh, under the Presidency of Sir A. Geikie.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 190. JULY. 1892.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, June 10, 1892.

E. B. Knobel, *President*, in the Chair.

Secretaries: H. H. Turner, M.A., B.Sc., and E. W. Maunder.

The Minutes of the previous Meeting were read and confirmed.

Seventy-six presents were announced; amongst them were specially mentioned:—H. C. Russell, 'Description of the Star Camera at the Sydney Observatory,' and enlarged photograph of the region surrounding η Argûs, presented by H. C. Russell; H. Jacoby, 'The Rutherfurd Photographic Measures of the Group of the Pleiades,' presented by the Author; '1st and 2nd Reports of the Solar Physics Committee,' presented by the Committee; 'Tables de logarithmes à huit décimales,' presented by the French War Office; Prof. S. Glasenapp, 'Mésures d'Étoiles doubles,' presented by the Author: two lantern-slides of the region surrounding Nova Aurigæ, presented by the Lick Observatory; Photographs of the globular cluster in Pegasus and of the nebula ♅ V. 15 near 52 Cygni, presented by Mr. Roberts.

Mr. Turner read *Mr. J. H. Poynting's* paper on a "Parallel-plate Double-image Micrometer."

Mr. Crommelin. In the April number of the 'Monthly Notices' Mr. Marth contributed a paper calling attention to a rather unusual phenomenon—the occultation of a bright star by Jupiter. The conditions were very unfavourable in England. Jupiter rose only one hour that day before the Sun, and the observation had to be made in bright twilight, with Jupiter only about 10° above the horizon. The instrument used was the 10-inch guiding telescope of the photographic equatorial at a much lower altitude than the apparatus was designed for, and we had some difficulty in getting it into adjustment, and all these circumstances combined to make it a very rough observation. The results, so far as they are worth anything, make the time of the central occultation about 11 minutes earlier than the time given in Mr. Marth's paper. He gives the time $15^h\ 36^m\ 6^s$; the mean time of the disappearance and reappearance gives $15^h\ 25^m\ 6^s$. The limb was boiling all the time, and it was difficult to say within 3 or 4 minutes when the star did disappear. If the difference of 11 minutes which has been observed is to be relied upon, it indicates a tabular error in the place of Jupiter of 6 seconds of arc, assuming the star places to be accurate.

Mr. Marth. I hope that perhaps next week we shall get news from the Cape, because I gave Dr. Gill sufficient notice to look out for the occultation. We shall then learn what they saw at the Cape under the more favourable circumstances under which the observations could be taken there.

The President. We have with us this evening Mr. Brashear, who has been visiting the observatories of Paris and Brussels, and perhaps he will give us an account of them.

Mr. Brashear, after explaining that he did not anticipate the pleasure of speaking to the Society that evening, said:—My first visit was to the observatory at Meudon, and there I was given a cordial invitation to visit the new observatory. I found that the cupola was about finished for the great equatorial. It is going to be a very fine piece of work when finished. The observer is going to be carried with the dome. The chair extends down to the side of the dome, and as the telescope moves the observer is moved round. After awhile we may have a machine for observing. [Laughter.] The great equatorial is well under way. I visited the shop of MM. Henri, and found that the tube was nearly finished. It looked like a great big steamship stack. There is to be a photographic objective on one side and a visual objective on the other. The photographic objective is finished, and it is 28 inches in diameter. The visual objective is well under way, and MM. Henri told me that it will be finished in about two months. If it is finished in that time they must be very rapid workmen, and in this they will soon get ahead of the Yankees; and if they do finish it within two months we shall have to give them the palm. There is no doubt, however, that these gentlemen do most excellent work. I had the pleasure of making an observation with the great equatorial coudé. The remarkable work of these gentlemen is shown here; and while they do not boast of anything great about this equatorial, I think they are right. They invited me to test it. I had the pleasure of observing γ Virginis, and while the great mirror showed deflection in one place the image of the star was well shown. The image of the star such as I saw there certainly speaks very well for the work; and although the definition was not good, yet I could eliminate that, as anyone who has used a telescope very much can do, and I found something very much better than I expected to see with the two mirrors and the objective, and yet the stellar image was very good. The photographic work of the Paris Observatory is going along nicely, and the great 48-inch is being remounted, and the Henri Brothers are going to refigure the present mirror, and if that does not answer they will make a new one. I have seen the work on solar prominences, and it is beautiful; and I think there is promise of great work in the future at the Paris Observatory.

M. Trouvelot is still doing his work on the solar prominences, and I heard a paper by him before the French Astronomical Society which was most interesting. I attended a séance there, and heard a paper read on sun-dials.

I had the pleasure of visiting the great observatory which is in prospect in Brussels. I met there Mr. Lancaster and the Director, but the Director was just interrupted by a gentleman who had come to see what was the matter with the telescope and was going to help him out of his trouble, and he turned me over to Mr. Lancaster, who took me round the great building. If they carry out what they propose it will be one of the finest institutions on the Continent. There are, however, some mistakes in the construction of the observatory—they have got it too high. However, I said I thought it was a mistake in the right direction, because they wanted a spectroscope. They have now four domes erected, and although they occupy 300 feet in every direction, you can go from one observatory into another through a beautiful underground archway, and everything is designed for the comfort of the astronomer; and I have no doubt that with such a good man at the head of the observatory at Brussels we shall hear of a great work being done there. I was much delighted with my visit to the Continent, but I must say they were all trying to kill me with kindness.

A vote of thanks was passed to Mr. Brashear for his statement.

Mr. Turner said that a beautiful enlargement of β Argûs had been sent by Mr. Russell from a photograph with $5\frac{3}{4}$ hours exposure; the scale was $12\frac{1}{2}$ inches to a degree, and was of a very satisfactory character. They had also three very good photographs presented by Mr. Isaac Roberts with his papers thereon.

Mr. Maunder then read a paper on "A List of Proper Motions in Line of Sight of 51 Stars," by *Prof. Vogel* (Foreign Associate), which he stated was a continuation of Prof. Vogel's communications of December 1891 on the spectrographic method. The complete discussion of results would be given in the publications of the Observatory. The paper simply gave the mean results of the stars and not the details. Each plate was measured by Prof. Vogel and Prof. Scheiner independently, and the separate results were given in the paper. In the majority of cases their conclusions agreed. In two stars, however, there was a discordance of 6 miles per second, but on the whole the agreement between the two observers was very close.

The President. There is hardly any name which is more frequently heard in connection with the list of papers received by the Society than that of Mr. Marth, whom we are pleased to see here to-night; and I would invite him to make some remarks upon his work. I believe he wishes to offer some remarks upon a drawing of the Milky Way.

Mr. Marth. I want to ask for some information referring to the question of Jupiter's satellites from anyone who knows something about Prof. Adams's work in connection with the satellites. Since I was here last year I have received a letter from Prof. Souillart. Those who take an interest in the matter will remember that I have accepted his coefficients for computing the position of the satellites. He had been in correspondence with Prof. Adams between the years 1878 and 1881, and Prof. Adams had been very kind in answering his questions; but he had not given the slightest hint that he had made new determinations of the masses of the satellites, about which Prof. Souillart first learnt anything from my paper published last year. I mentioned that there was still some hope of recovering Delambre's manuscripts, from which one might learn what has been really done in forming the tables of Jupiter's satellites. Mr. Tisserand and Prof. Souillart, however, have searched in vain for these papers. The theory of Jupiter's satellites was published in the 4th volume of the 'Mécanique Céleste' in 1805, from which we merely learn that the data upon which they are founded were supplied by Delambre; but how Delambre has found his coefficients nobody knows. The whole theory will have to be worked out from the beginning. I should like to know to whom I may appeal in Cambridge as likely to know what Prof. Adams himself may have done in the matter, and who is responsible and who has seen the manuscript. I should like to have some information on this point. The matter is in a most unsatisfactory state. Every other branch of the science has made progress, but in this case for 90 years observations have been made, and have not yet served for the improvement of the tables. Perhaps Mr. Turner can tell me to whom I can apply?

Mr. Turner. I happen to be able to speak with some knowledge of this subject, because the question of Jupiter's satellites was one which I raised myself with Professor Adams in a few conversations I had the privilege of having with him within the last year or two before his death. When his health began to fail I hoped at one time to take off his hands some of his work that might be ready for publishing, and one of the first things I asked him was whether his work on Jupiter's satellites, which I heard was nearly completed, was so nearly ready for the press that it could be entrusted to me. He very eagerly brought down the volume of Souillart, and made some remarks to me upon the whole question; one was that he considered the theory of Souillart defective, in that he had considered the value of the masses required correction, and had introduced corrections accordingly; but he had introduced them in one place and retained the old masses in another, and consequently the theory required correction. He went on to say that some of the most important information to be got would be obtained by observations of the eclipse of the third satellite. He pointed out the way in which these phenomena were specially important. Then I asked him if this work was in such a form that it could be easily printed; but he and Mrs. Adams also expressed the opinion that it was not in such a form. These matters had been worked out by Professor Adams, but the work could hardly be translated by anyone not acquainted with his methods. Since the lamentable event of his death I believe his manuscripts have been placed in the hands of one or two people, but I do not know how far I am at liberty to mention the names of those who are examining them. They are finding them in very much greater order than Mrs. Adams expected at one time, and I hope that Mr. Marth will be answered in a much more effective way in a few months, or perhaps weeks.

The President. I think our thanks are due to Mr. Marth for having brought forward a question which has elicited such an interesting answer from Mr. Turner.

The President then read a circular which the Council had received relating to the scientific section of the Chicago Exhibition, and which the Council had requested him to communicate to the Fellows.

Mr. Chambers. May I ask what the Council suggest?

The President. The Council do not make any suggestion on the point. They thought it was hardly a subject which could be dealt with by them as a Council, and the only thing possible was to bring this communication to the notice of the Fellows of the Society. [We give this circular further on.]

Commandant Defforges gave (in French) an interesting account of his pendulum operations, in continuation of what he had said three years ago. In those three years they had learnt many things. He had spoken of the information given by the pendulum as to the figure of the Earth; they had learnt that the pendulum would determine most things *except* the figure of the Earth. There were many sources of error, and he would speak of two or three. It was important to swing the pendulums in vacuum, as nearly as possible, so that the oscillations might continue undiminished in amplitude for some time; and one great difficulty was to retain the vacuum when once air was exhausted. After many trials they had found the best thing to be cab varnish; and when this was used the exhaustion would remain at a pressure of 1½ millim. so well that the amplitude took 72 hours to diminish from 2° to 2′. With regard to Sir G. Stokes's theoretical formula for the resistance of the air, they had not only confirmed it by experiment, but found in addition that the μ in the formula (the coefficient of internal friction) was a constant for all gases.

Commandant Defforges then explained the integration of the formula for the amplitude of vibration, under certain conditions.

He then dealt with the form of the knife-edge supports, and explained how the edge was really a succession of facets—was, in fact, a prism by construction; and this form could be detected by experiments, in which what should be a continuous curve was found to be a succession of steps like a staircase. He described the method of eliminating the movement of the supports.

With regard to the general results arrived at, it was found that Clairaut's law required considerable modification. Weight was really a function of the crust of the Earth in the vicinity. Gravity is larger over the ocean (as on a small island) and smaller on continents than this law indicates. It is reasonable to suppose that the crust is denser where it has shrunk to form hollows than where it is expanded into mountains; and probably this was the true reason of the apparent anomaly.

The President. I would offer M. Defforges our very best thanks for his very interesting statement.

Mr. Turner. I am engaged at present, under the superintendence of the Astronomer Royal, on various longitude operations, and one of them is of slightly greater interest than usual, and I thought, therefore, I might say a few words about it to-night. At present Commandant Defforges and I represent one end of a longitude which is being determined—that of Paris; and we have had such fine weather during the last three nights that we are taking a holiday to-night to recuperate. But we are also at Greenwich conducting three other longitudes—those of Montreal, Canso (which is the end of the cable connecting Montreal with Greenwich), and Waterville (the other end). Longitude stations are in existence at all those places. The first part of the operations was completed a few weeks ago, and the second part will be begun in the autumn. The visit to Waterville (which is near Valencia) has been of extreme interest. The station is important as being the end of the great European arc of longitude extending to Orsk in the Oural Mountains, which will be of value in fixing the size and shape of the Earth, a matter which, as Commandant Defforges has said, the pendulum is somewhat unsuited for. The longitude of Valencia was attended to many years ago, and in some sense we are repeating those observations; but for various reasons we do not expect to get the same result, even though we may be successful. We might, for instance, find, as we have found before, that the accuracy of longitude observations is not such as at one time we hoped; the time-determination at each end is found to be more difficult than was thought. If we find our longitude differs from previous determinations, it may be that one or other time-determination has had some systematic error. Or it may be that local attraction has some influence upon the results. To determine this the Astronomer Royal has suggested our establishing another local station next year, and testing the influence of the form of the ground. So much for the time-determination. What chiefly interested me was the cable-signalling, which is peculiar in this longitude operation between Canso and Waterville. [Photographs on screen of buildings of Waterville, and of the telegraph-signalling.] We had at one time decided to use the siphon-recorder as a relay; it was tried in the preliminary signals last year, when the best method was under discussion; finally, the siphon-recorder itself was used as a chronograph. We have the means of getting extreme accuracy out of the trace, because the siphon is agitated by a tuning-fork; each dot of this trace represents the same fraction of a second. We were able to read all the traces with extreme accuracy. Last year we measured the actual passage of time from Waterville to Canso and back. The recorder at one end was converted into a relay, and allowed to repeat the signal back again, and we were able to send out a signal from Waterville and get it back automatically, and we found the time occupied in travelling one way was $0^{s}{\cdot}2$, and that we found agreed with Sir Wm. Thomson's remarks in a paper to the Society of Engineers some years back. Our signals, which have been made this year on another plan, confirm that result. The first part of the longitude consisted of exchanges between Greenwich and Waterville, and Montreal and Canso, but as yet no observer has crossed the Atlantic. In the autumn Prof. McLeod and I hope to exchange, and he hopes to be here in the summer to discuss such results as are already obtained.

Thanks were passed to Mr. Turner for his statement.

The President. There is one subject the photographic Committee are considering very carefully, and that is the reproduction of photographs; and Dr. Spitta has been kind enough to consent to show us a couple of results which he has obtained, and which will be very interesting to the Fellows.

Dr. Spitta. I do not propose to say anything upon the subject of these lantern-slides, but rather about the way in which they are produced. Our Council is a progressive one, and now that we have started a lantern, every one is desirous of using it. Every one, too, is desirous of showing the best slides he can, and all who make the attempt to show real black and white results will corroborate me in acknowledging how difficult they are to produce. Indeed, with the ordinary lantern-plate used with a dry negative, I am *almost* tempted to say it has not been done. It may be remarked that it does not matter whether you have black and white results or not, but in reproducing pictures of the description I hope to show you in a moment, it really makes all the difference, a brown background quite spoiling the general effect. I set myself the task of doing this, if posssible, and after a great many experiments with several kinds of lantern-plates, have found that it is simplicity itself when using Paget ordinary plates and employing hydrokinone as developer. One great difficulty in making this type of picture, where a background of pure grey is required, is to get rid of minute spots. These arise from two sources—particles in the film, and particles which fall on the plate before development. With the particles in the film I have nothing to do, but for those which adhere to the plate from the dark slide, &c., I can suggest a most simple remedy. One has been always told to dust the plate before exposure and before development; but this dusting the plate when in the developer is a frequent source of minute scratches, which do not matter on an ordinary negative, but which show in a lantern-plate when thrown on the screen. The simple remedy is to tap the plate sharply on a board before putting it in the slide, and before placing it in the developing-dish. Potash developer with hydrokinone, although not so good for celestial work, is best for this kind of plate, and it is most suitable for intensification by Monckoven's excellent formula with cyanide of silver.

Mr. Wesley then explained the figures thrown upon the screen.

The President. I think the result shows how excellently Dr. Spitta has succeeded in reproducing drawings for lantern-slides, and I am sure our thanks are due to him for bringing this before us.

Mr. Turner then read a paper by *Mr. Stone* on the opposition of Mars, 1892. The paper referred to the fact that a circular had recently been issued from the Washington Observatory with regard to the redetermination of the solar parallax. After making some remarks on the circular, Mr. Stone remarked that the necessary co-operation had been rendered doubtful by the shortness of the notice given, which rendered any modification of detail difficult. The method proposed to be adopted would require that a system of parallel wires should be used; and it was also proposed in the circular to use a reflecting prism outside the eyepiece. He feared that the attempt to carry out the scheme would lead to great systematic errors. He (Mr. Stone) proposed to observe Mars and the comparison stars in the usual way with a single wire and with a prism, and he hoped that any directors of southern observatories who might not be prepared to adopt the Washington scheme would observe the stars in the same way. "The paper," Mr. Turner said, "is of some importance, because the opposition is very close, and any scheme of co-operation will fail unless a sufficient number of observatories join in it, and Mr. Stone's opinion is of great weight, he being at the head of a meridian observatory."

The President. It seems to me that this paper is so very important that we ought to print it and distribute copies to observatories that are contemplating taking part in this work, as soon as possible. If we wait until it is published in the 'Monthly Notices,' a good deal of its value may be lost.

The following papers were announced:—

E. Crossley and *J. Gledhill.* "The Lunar Eclipse, 1892, May 11, observed at Mr. Crossley's Observatory."

J. H. Poynting. "On a Parallel-plate Double-image Micrometer."

Rev. A. Freeman. "Occultations and other Phenomena of the Lunar Eclipse, 1892, May 11."

Prof. S. Glasenapp. "Orbit of 9 Argûs (β 101)."

Prof. H. C. Vogel. "List of the Proper Motions in the Line of Sight of 51 Stars."

J. E. Gore. "The Orbit of OΣ 269."

Cambridge Observatory. "Observations of Wolf's Periodical Comet."

Isaac Roberts. "Photograph of the Nebula ♅ V. 15, near 52 Cygni."

Isaac Roberts. "Photographs of the Region of the Globular Cluster 15 M. Persei."

Capt. G. T. Parsons. "Sextant Observations of Swift's Comet."

Royal Observatory, Cape of Good Hope. "Observations of Comet *a* 1892 (Swift)."

Royal Observatory, Greenwich. "Observations of Swift's Comet (*a* 1892)."

Royal Observatory, Greenwich. "Occultation of 73 Piscium by Jupiter, 1892, May 23."

Prof. S. Glasenapp. "Orbit of the Double Star κ Pegasi (AC)."

A. Marth. "Data for computing the Positions of the Satellites of Jupiter, 1892."

A. M. W. Downing. "Note on Damoiseau's 'Tables Écliptiques des Satellites de Jupiter."

H. C. Russell. "Photograph of η Argûs."

Capt. G. T. Parsons. "Sextant Observations of Swift's Comet."

E. J. Stone. "The Opposition of Mars, 1892."

Rev. James Baikie, Wood Inch, Dalguise, near Dunkeld, Scotland; *John Dansken*, 4 Eldon Terrace, Partick, Glasgow; *Henry Daniel McCarthy*, Cheltenham; *Martin Charles Sharp, M.A.*, 5 Portman Street, Portman Square, W., were duly elected Fellows of the Society.

The following Candidates were proposed for election as Fellows of the Society:—

John A. Brashear, Astronomical and Physical Instrument Maker, Allegheny, Pennsylvania, U.S.A. (proposed by W. H. Maw); *Walter William Bryant*, Assistant, Royal Observatory, Greenwich, Seymour Cottage, Conduit Vale, Blackheath, S.E. (proposed by W. H. M. Christie); *Captain James Fisher*, 7 Fenchurch Avenue, E.C. (proposed by P. Thompson); *Thomas Charlton Hudson*, Assistant, Royal Observatory, Greenwich, 88 King George Street, Greenwich, S.E. (proposed by W. H. M. Christie); *Francis R. Wardle*, Banker, 43 Moorgate Street, E.C. (proposed by H. B. Chamberlin).

ROYAL METEOROLOGICAL SOCIETY.

THE last meeting of this Society for the present session was held on Wednesday evening, June 15th, at the Institution of Civil Engineers, 25 Great George Street, Westminster. Dr. C. Theodore Williams, President, in the Chair.

Mr. T. W. Backhouse, F.R.A.S., Lieut. H. M. Lambert, R.N.R., and Mr. W. Topley, F.R.S., were elected Fellows of the Society.

The following papers were read:—

1. "English Climatology, 1881–1890", by Mr. F. C. Bayard, F.R. Met. Soc. This is a discussion of the results of the Climatological Observations made at the Society's stations, and printed in the 'Meteorological Record' for the ten years 1881–1890. The instruments at the stations have all been verified, and are exposed under similar conditions, the thermometers being mounted in a Stevenson screen with their bulbs 4 feet above the ground. The stations are regularly inspected and the instruments tested by the Assistant Secretary. The stations now number about 80, but there were only 52 which had complete results for the ten years in question. The author has discussed the results from these stations and given the monthly and yearly means of temperature, humidity, cloud, and rainfall. His general conclusions are:—

(1) With respect to mean temperature the sea-coast stations are warm in winter and cool in summer, whilst the inland stations are cold in winter and hot in summer. (2) At all stations the maximum temperature occurs in July or August, and the minimum in December or January. (3) Relative humidity is lowest at the sea-coast stations and highest at the inland ones. (4) The south-western district seems the most cloudy in winter, spring, and autumn, and the southern district the least cloudy in the summer months, and the sea-coast stations are, as a rule, less cloudy than the inland ones. (5) Rainfall is smallest in April and, as a rule, greatest in November, and it increases from east to west.

2. "The Mean Temperature of the Air on each day of the year at the Royal Observatory, Greenwich, on the average of the fifty years 1841 to 1890," by Mr. W. Ellis, F.R.A.S. The values given in this paper are derived from eye observations from 1841 to 1848, and from the photographic records from 1849 to 1890. The mean annual temperature is 49°·5. The lowest winter temperature, 37°·2, occurs on January 12th, and the highest summer temperature, 63°·8, on July 15th. The average temperature of the year is reached in spring on May 2nd, and in autumn on October 18th. The interval during which the temperature is above the average is 169 days, the interval during which it is below the average being 196 days.

*Astronomical Possibilities at Considerable Altitudes**.

As considerable attention has been directed of late to mountain observatories, and as the Boyden Station is, I believe, the highest astronomical observatory in the world having such an extensive equipment for the study of the heavens, a brief description of the atmospheric conditions found in this locality may be of general interest.

The Boyden Station of the Harvard College Observatory is situated two miles from the city of Arequipa, Peru, in latitude 16° 24′ south, and longitude $4^h\ 45^m\ 30^s$ west from Greenwich, and at an altitude of 8060 feet (2457 metres) above the sea-level. As an account of this station and its equipment will be given later in the 'Sidereal Messenger,' the present article will be confined strictly to a description of the atmospheric conditions under which the work is carried on. As we arrived here at the end of January 1891, it is impossible at present to give a complete account of the year's climate. Apparently, however, it may be described as follows:—The rainy season begins at the end of January and lasts through March, with a rainfall of about three inches. It is then clear, with comparatively small interruption from clouds, until the following November. A cloudy season then sets in, lasting a month, during which there was this year a rainfall amounting to 0·01 inch on two successive days. During the two remaining months we expect clear weather perhaps half of the time. The thermometer here rarely falls below 40° Fahrenheit, and rarely exceeds 75°. The extreme fluctuations of the barometer amount to rather less than 0·2 inches.

The Observatory is situated upon the crest of a hill overlooking the valley, and at an altitude of 400 feet above the city. It is overshadowed by three extinct or nearly extinct volcanoes, rising to altitudes varying from 18,500 to 20,000 feet above sea-level, but in no case exceeding 12° altitude as seen from the Observatory. Towards the south-west the horizon is clear, down to a level with the eye.

The first fact which strikes the attention of an observer coming from a sea-level station in the temperate zones is the brilliancy of the stars. Night after night 6·5 magnitude stars are picked out with the naked eye without the slightest effort. The eleven Pleiads can always be counted when the Moon is not too bright. The Andromeda nebula, seen with difficulty at home, is here a much more brilliant object, appearing larger than the Moon. 3·0 magnitude stars have been seen to disappear below the horizon where the latter was on a level with the eye, while 4·0 magnitude stars disappear at perhaps half a degree higher up. Every night the faint hazy light of the zodiacal arch spans the horizon from east to west, yet is so conspicuous that on pointing it out to one of our Peruvian assistants, with the request hat he should observe it, he remarked that he had often noticed it, and supposed it was part of the Milky Way. The Gegenschein is readily seen any evening after nine o'clock, and we have on several occasions used it as a convenient reference-mark to determine roughly whether a celestial body has reached opposition with the Sun or not. One of my assistants, Mr. A. E. Douglass, thought one evening that for a short time it was as bright as the Lesser Magellanic Cloud. One advantage of our location is that if a difficult phenomenon is observable one night, we know that it will in general be equally visible night after night, with but little interruption, for weeks at a time. In the case of the Gegenschein, however, a certain irregularity in its brilliancy seems to occur.

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* From the 'Astr. Nachrichten,' No. 3079.

The same atmospheric transparency is noted with the 13-in. Clark refractor. As an illustration of this, while in the temperate zones little can be seen of the great Orion nebula that is not figured in Bond's drawing, here the whole of the photographic region, first shown in the Harvard photographs of 1887, and exhibited at the meeting of the first Astrophotographic Congress, is clearly visible to the eye, and could not escape detection even by one wholly unfamiliar with its conformation. This addition more than doubles its apparent area, and renders it indeed the most splendid object in the stellar universe.

Nor is it only in atmospheric transparency and uniformity from night to night, that a gain is made by ascending to a moderate altitude in the tropics. The stars appear here with a steadiness and lack of fluctuation, which is a very severe test upon the optical qualities of an instrument. A scale of steadiness of seeing has been adopted, on which 3 indicates that the brighter stars are surrounded by numerous diffraction rings, but that they are broken and impossible to count. 4 indicates that they are complete and may be counted, and that the central point of light is readily separable from the inner ring, while 5 indicates that each ring is perfect and immovable. As it is impossible to detect any improvement upon the latter condition, I have denominated it as "perfect" in my records. Usually half a dozen rings may be seen about the brighter stars; but when the seeing is "perfect," as many as twelve have been counted around α Centauri. This condition it should be stated has been noted on but two or three occasions this year; but then much of the work has been done at a considerable distance from the zenith, and visual observations were carried on for but little over three months. At the end of that time the crown lens was reversed, and the instrument used for photographic purposes. The seeing is generally divided between 3 and 4, and certain subdivisions have also been formed. It is so rarely inferior to 3, that no distinct definitions have been given to the lower numbers of the scale; but when the star images increase in size beyond mere points, or otherwise indicate inferior definition, the instrument has been closed or turned to other uses.

The rings indeed are usually so distinct, that they seriously interfere with the detection of the faintest companions to bright stars; but if the companion is bright itself, as in the case, for instance, of α Scorpii, we have perhaps six rings around the primary, and one intersecting them around the companion.

In examining surfaces, such as the bodies of the solar system, it was frequently impossible to detect any wavering whatever of the edges of the disc, even with a power of 400, which was the one always employed with the 13-in. Indeed, it was impossible to distinguish any difference in the sharpness of the limb as seen in the 13-in. and as seen in the 8-in. finder, with a power of 60. Until recently a one-half inch eyepiece was the shortest one in my possession, which explains why higher powers were not tested and employed.

Several facts have already been noted here which I believe to be new, but they cannot well be included within the scope of the present article. I will mention, however, an observation, which, while it brings out no new facts, will illustrate very well the character of the definition. Shortly after the telescope was erected about the middle of August, I was examining Jupiter's third satellite, which under the power employed appeared somewhat smaller than our own Moon with the naked eye, and nearly equally distinct. Near the centre of the disc, but not quite coincident with it, a small dark spot was observed, with rather hazy edges. This was at once set down to planetary detail, and careful drawings were made. The next night it was observed again, in the same place, but rather smaller, and again the following night. No motion of rotation, however, could be detected. The observations were continued, and the other satellites also observed. One particularly fine night the spot was very distinct and very small, appearing but little larger in proportion to the disc than the Mare Crisium does upon the Moon. On turning the telescope to the fourth satellite an exactly similar spot was seen, and similarly placed. Then it flashed upon me that what I was observing was a diffraction phenomenon, the size of the spot depending on the character of the seeing, and its non-coincidence with the centre of the disc being due to some slight lack of adjustment in the lenses. Some weeks later I was examining Jupiter. The seeing was very good, and the fourth satellite was in transit, appearing as a round dark spot. To my surprise I noticed that near the centre of its disc was a comparatively brilliant point of light. At first my other observations did not occur to me, but I presently saw that what I was now observing was merely the diffraction spot reversed, owing to the bright background of the planet. Indeed this same phenomenon has been frequently observed at the time of a transit of Mercury, but I do not know of its having been before noted in the case of a transit of Jupiter's fourth satellite. It is quite possible that these diffraction spots may in part explain the somewhat confused accounts of observed markings upon Jupiter's satellites. They are certainly much their most conspicuous features, since their surfaces are far more free from genuine planetary detail than is the case with our own Moon.

In closing this article I wish to express my opinion, that as the result of my experience with large refractors at altitudes varying from 6000 to 14,000 feet, among the mountains of Colorado, California, and Peru, that for transparent skies one must approach the tropics, and that for steady seeing one must have an extremely dry climate. Contrary to the not uncommon experience in localities when the seeing is poor, the thinnest haze here at once reduces the quality of our seeing, although perhaps still leaving it much better than it would be under similar conditions at home. For our very best seeing the stars must be brilliant, and there must not be the faintest trace of a cloud in the sky. In short, moderate altitude is, I think, a most desirable qualification for an observatory, but suitable latitude and habitual dryness of climate are still more important.

Arequipa, Peru,
1891, Dec. 9.

W. H. Pickering.

Nova Aurigæ and its Suggestions.

The one certain result of investigations into the late stellar outburst is that it was occasioned by the mutual disturbance of two cosmical masses rushing past each other at high speed. It can scarcely be maintained, however, that this conclusion applies exclusively to the single instance in which it has been demonstrated. There is, to say the least, a strong presumption that it holds good for all objects of the same class. But if for all Novæ, why not for variable stars as well? No really fundamental distinction can be established between one kind of phenomenon and the other. "New stars" approve themselves as such only negatively. The mere fact of the reappearance of any one of them would at once compel its transference to the ranks of variables. And the subsisting difference, such as it is, becomes still further attenuated on consideration of the abortive periodicity shown by Anthelm's Nova of 1670 in its twofold revival at annual intervals. Nor can spectroscopic tests be much relied upon, though some may, with qualifications, be indicated. Thus, the typical bands of the third stellar type, with superposed bright hydrogen-lines, characterize, perhaps unfailingly, stars subject to wide fluctuations of lustre in periods of many months; but shaded zones less regularly disposed were observed in the Nova of 1866, and are included with bright lines in the spectra of such variables as R Coronæ and R Andromedæ. Again, the vivid emergence of the solar chromospheric lines at 502 and 492, as well as of the calcium-lines H and K, does not appear, so far, to have been recorded in any but temporary stars;

nevertheless, it is far too soon to claim it for them as an exclusive feature.

If, then, temporary form with variable stars one continuous series, we cannot easily suppose their augmentations of light produced by radically different means. The most obvious explanation of their relationship is that the bodies giving rise, by their conjunction, to a solitary blaze, pursue hyperbolic orbits, and hence meet once for all; while periodical variability depends upon the elliptical revolutions of a permanently united pair. It is practically certain, at any rate, that the components of Nova Aurigæ traversed hyperbolas. Assuming their nearest approach to have occurred at the epoch of their maximum brightness, on December 20, 1891, they had been in course of separation during sixty-four days, when, on February 22, 1892, a photograph of their spectrum was taken at Tulse Hill showing a relative velocity of about 550 miles a second. Moreover, this speed gave, down to the end of the month, no sign of slackening determinable with certainty by the Potsdam spectrograph. Now so persistently swift a movement cannot have been either elliptical or parabolic—cannot, in other words, have been due solely to the attractive force of the bodies it belonged to. For this reason, that a mass incredibly great would have been needed for its generation. Ten thousand times that of the Sun would certainly have been inadequate for the purpose, as a rough and ready calculation shows. The extravagance of this minimum-estimate compels the adoption of the alternative supposition that the movement of the temporarily conjoined stars was of a hyperbolic nature. It was, accordingly, in large but undetermined measure, inherent, and may have included but a comparatively insignificant element due to the accelerative power of gravity.

This conclusion might probably be extended to other similar cases, but no reason is apparent why the pair constituting a new star should invariably possess the curiously matched, yet contrasted spectra visible in Nova Aurigæ. Nor, if they did, could such spectra often be displayed to the same advantage. Their separate study was made possible, in the recent apparition, only by the disjunctive effects of extraordinarily swift radial movements. Apart from those effects, they must have been seen blended together into a confused amalgam of bright and dark lines, partially neutralizing each other by their coincidence. A spectrum not unlike that of Nova Andromedæ, or of U Geminorum, might conceivably have resulted from the mixture. The chances, however, are very great against the recurrence, in any future new star, of the conditions fortunately exemplified by Nova Aurigæ. Not only must the orbits traced out by the connected bodies have lain nearly edgewise to our view, but the axes of the same orbits must have been directed nearly towards the Earth. Otherwise, the sections of them in which radial movement predominated would have been traversed very quickly, and left far behind before spectroscopic measurements began to be made.

Abundant opportunities, however, are at hand for enquiry into the systematic relations of stars chronically variable. They exist in such numbers and of such diversity that, if each actually represents a pair of bodies circulating in the period of light-change, decisive evidence to that effect can scarcely fail, with proper diligence, to be quickly elicited. The orbital movement of long-period variables like Mira Ceti cannot indeed be expected to prove exceedingly rapid. Hence, the spectrographic determination of its radial component may prove to be a somewhat delicate operation, which must often be further complicated by the presence of translatory motion. But the investigation possesses so high an interest that it should be attempted in spite of whatever difficulties may stand in the way.

Nevertheless, the spectrum of Mira, the typical long-period variable, contains one rather significant indication of singleness of origin. The series of bright hydrogen-lines illuminating it is broken by the effacement, through strong and diffuse calcium-absorption, of the ray near H. Now this certainly does not look as if the bright lines were emitted by one body, while the absorption was produced in the atmosphere of another. If this were so the integrated light should contain some trace, however feeble, of the missing line. Its apparent annihilation seems, on the contrary, the result of the overlaying, in one and the same stellar atmosphere, of erupted and glowing volumes of hydrogen by a cooler stratum of vapour of calcium. The point is of crucial importance, and ought to prove capable of being decided, by photographic means, with comparative facility.

One stellar spectrum, however, is already known to present a most remarkable analogy to that of the lately-vanished Nova; and it belongs, singularly enough, to a short-period variable. Until further details regarding the spectral changes of β Lyræ have been published no settled opinion can be formed as to their cause; but the shifting of bright hydrogen-lines so as to become alternately more and less refrangible than corresponding dark ones in a period strictly synchronizing (in the long run, at all events) with the star's period of light-change, leaves little or no doubt that a binary system is in question, the revolutions of which in some way prescribe the law to the fluctuations of brightness. Changes in the character of the lines accentuate the resemblance to Nova Aurigæ. Thus, the brilliant rays at times appear double; and if, as Dr. and Mrs. Huggins suggest with much probability, this effect was, in the Nova, due to such "reversals" as constantly occur above sun-spots, it should naturally meet, in the variable, a similar explanation. It is not, indeed, quite easy to understand how the violent disturbance of the light-giving apparatus likely to be associated with the production of reversals on the large scale necessary to render them conspicuous can be made compatible with the uniform course of change pursued by the Lyre variable. Yet this seeming anomaly may eventually find an explanation in connection with a circumstance unexpectedly brought out by Professor Pickering's photographs. They seem to show—so far as written statements about them enable one to judge—that the two unequal minima characterizing this star take place just when mutual occultations by the two bodies contributing to its light would be possible—that is, just when they cross, in opposite directions, the line of sight from the Earth. This relation, if confirmed, can hardly be accounted for otherwise than by a double eclipse, to which, indeed, M. Plassmann had recourse some years ago, in combination with tidal agitations, as a means of explaining the variations of β Lyræ. The curve graphically expounding them flows, nevertheless, with perfect smoothness and continuity. It presents no feature suggestive of the abrupt intervention of an opaque mass. The recently-discovered example of 11 Monocerotis shows, however, that a revolving combination like that of β Lyræ is not necessarily attended by fluctuations in lustre. Some further condition must be present in order to produce them. The spectrum of this 4·2 magnitude star has been found, during the progress of Mrs. Fleming's examination of the Draper Memorial photographs, to contain a bright line displaced from the blue to the red side of F in a period yet to be ascertained The results of the special investigation set on foot on the detection of this change will be awaited with great interest. Meanwhile the suspicion naturally presents itself that the spectra of such stars as γ Cassiopeiæ, P Cygni, and Pleione may be similarly compounded, although, for want of the analysing agency supplied by motion in the line of sight, their elements may not prove readily distinguishable.

AGNES M. CLERKE.

Selenographical Notes.

PLATO.—Professor Pickering's article in the 'Observatory' for June, so far as it relates to the minute details on the floor of this formation, revives a question which, despite the increase in the number of lunar observers and of instruments sufficiently large to cope with the difficulties of scrutinizing features of such a delicate and evanescent character, has practically lain dormant for nearly a quarter of a century. As one who for some time took part in the systematic observation of the crater-cones, light spots, and markings in the interior of Plato, which are recorded in the British Association Reports for the years 1871 and 1872, I rejoice that after so long an interval these phenomena have attracted the attention of so able and experienced an observer, using a 13-in. Alvan-Clark under exceptionally favourable atmospheric conditions.

In the absence of details as to time and phase, a satisfactory discussion of Professor Pickering's observations is not possible.

Something may, however, be said about the whilom crater-cone 4, which, it is asserted, has entirely disappeared, leaving nothing to mark its place but a whitish stain. This crater, the most easterly of the triangularly-arranged group of crater-cones in the central region of the floor, is said to have been first seen as a light spot by Gruithuisen, but it has long seemed to me that he was forestalled by Schröter of Lilienthal. In the 'Selenotopographische Fragmente,' vol. i. p. 329, it is recorded that at 8^h on October 15, 1788, the latter observed in his seven-foot reflector, power 161, an ill-defined shimmer or lustre about 7 or 8 seconds in greatest diameter, south-east of the centre of the formation, the Moon at the time being some 15 hours past full. He saw the same appearance still more distinctly in the same situation at 8^h on October 4, 1789, the phase being nearly identical. Two drawings of Plato, showing this spot, are given in Tab. xxi. figs. 6 and 7. Now, if we bear in mind the inferiority of Schröter's telescope, it will, I think, be admitted that what he records can hardly have been any portion of the extremely faint marking known as the "sector," which extends like a fan from the neighbourhood of 4 to the south-east border, but was much more probably a white patch on or near the site of this crater, the visibility of which happened to be at the time, from some unknown cause, more pronounced than that of any other object on the floor. The anomalies presented by this spot, whether we regard it as a small light area or as a crater-cone, are so noteworthy and curious that I will briefly select from the observers' notes, as published in the British Association Reports, a few records relating to it.

During a very favourable view of sunrise, 1870 January 10, Mr. Gledhill saw, with the Crossley equatoreal of 7·3 in. aperture, 1, 3, 30, 4, 7, 9, 11, 17, and 32 as crater-cones, 1 being very lofty and prominent at 8^h 5^m, while at 8^h 15^m 4 was difficult and hazy. On April 9, 1870, 23 hours after sunrise, 1, 3, and 4 were noted by me as being very easily visible, as light spots, with a power of 200 on a 4-in. Cooke achromatic. At sunrise, 1870 February 9, Mr. Gledhill saw only the bright west wall of 4, and remarked "this object appears to have *lower* walls than 1, 17, or 3." He subsequently noticed that it became *indistinct*, and that there was "a dull yellow patch about it." On another occasion he writes "spot 4 never looks like 1, 17, or 3," and again, 1870 September 7, he describes 1 and 3 as "fine sparkling discs," and 4 as "a hazy spot." On the following night Mr. Neison saw 4 as a flat indistinct spot. Under date 1870 June 13 Mr. Gledhill remarks, "For some time I have thought that when power 115 was used spot No. 4 was almost at any time to be seen, or, at any rate, a condensation of the 'sector' at its apex was seen. On applying 240, however, the appearance vanishes, and no condensation or spot is seen, or perhaps only sometimes and at intervals." On August 23, 1869, when most of the easier spots were very bright as compared with their usual appearance, Mr. Pratt, using an 8½ S. O. G. reflector, noted that

No. 4 was not well defined; there was a persistent oval light round it (N.W. and S.E.), and he several times believed it to be double, but could not be positive that it was so.

If space permitted it would be easy to multiply instances, both from these Reports and from other sources, all tending to show that the object 4, though classed with the other crater-cones on the floor, has, from the time it has been systematically observed, exhibited peculiarities of appearance and visibility which have not been displayed by its neighbours. Though with an 8½-in. S. O. G. reflector I have frequently seen 1, 3, and 17 under a low sun standing out in the grey plain as bright elevated craters, it has rarely happened that any indications of a raised wall or dark centre could be detected in connection with No. 4. The phenomena which this object has presented in the past render the confirmation of Professor Pickering's statement as to its entire disappearance as a crater a matter of extreme interest and importance to Selenography. It is to be hoped that his notes relating both to this and the other spots he refers to will lead to a revival of the Plato observations on a more extended scale and with the aid of larger instruments.

With regard to the detection of the recorded spots and markings on the floor of Plato, I would remark that they appear to be much more easily traceable in a good achromatic of from 4 to 6 in. aperture than in a S. O. G. reflector of equal capacity. Though I have nothing but good to say of my 8½-in. reflector, an excellent 4-in. Cooke refractor, fixed on the same mounting, generally shows these details much more satisfactorily, and, judging from observations received from others, it would seem that my experience is by no means peculiar, for many using reflectors of about the same size as my own, evidently have great difficulty in discerning these features.

FRA MAURO B.—*Erratum.* In last line but one ('Observatory,' No. 189, p. 258), for "south" read "north."

Beaumont House, Shakespeare Road,
Bedford, 1892, June 19.

THOS. GWYN ELGER.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The Craterlets on the Floor of Plato.

GENTLEMEN,—

The very interesting article by Professor W. H. Pickering in the June number of the 'Observatory,' detailing the results of his researches upon certain selected regions of the Moon, will doubtless have been read with great zest by many of your readers. It is very seldom that a telescope so large as the 13-in. refractor employed is directed to the study of the Moon at all, and in the present case the results are particularly interesting and valuable on account of the very perfect atmospheric conditions under which the researches were carried on, and from their having reference to certain regions of the Moon of more than usual interest. This is especially the case with the floor of Plato. Probably no district of the Moon has received such close and continuous attention as this. Since the extensive series of observations made by a number of observers in the period 1869–71, and which have been elaborately discussed by Birt, the floor of Plato has by no means been neglected. In particular, from the year 1879 up to the present time, it has been, with a few short breaks, continuously under examination, and a great many additional details have been brought to light *. The following remarks upon some of the changes reported by Prof. Pickering will perhaps therefore be of some interest.

The most remarkable of these changes is undoubtedly the disappearance of the crater No. 4, situated a little to the east of the centre of the floor, since not only has it *often* been seen crateriform, but it was certainly one of the largest objects of its class upon the floor. Hence, if it should be found that the object can no longer be discerned as a crater, when examined under various degrees of illumination, then the long disputed question of real change on the Moon will be decided beyond dispute in the affirmative. In view of the importance of the subject, I have therefore extracted the following recent observations of No. 4 as a crater. Except where otherwise stated, they were made by the writer.

1880 Feb. 24, $11^h\ 0^m$.—In a sketch made by Mr. T. P. Gray with an 8-in. reflector, spots 1, 3, 4, and 14 are shown as craters.

1880 March 19.—The Rev. F. B. Allison saw the craterlets Nos. 1, 17, 4, 3 distinctly in the order of distinctness above, with a 4½-in. refractor.

1882 Oct. 2, 13^h.—Definition very fine. Spot No. 4 was the brightest on the floor, and was "distinctly and easily seen as a crater, with plain raised wall." 5¼-in. Calver, power 150. Neither the large central crater No. 1 nor No. 3 were visible as craters on this night.

1882 Oct. 29, 12^h.—Spots 1, 3, 4, and 17 are drawn and described as craters. 5¼-in. Calver, 150.

1887 July 12, $14^h\ 15^m$.—"4 evident, and is clearly a crater." 6½-in. Calver, 150, 225. Spots 1 and 17 were also seen as craters, but not No. 3, which was difficult.

1887 Sept. 8, $14^h\ 17^m$.—"4 a crater, but hazy and ill defined." 6½-in. Calver, 150, 320. Spots 3 and 30 were both visible as craters, but 31 was not seen.

1888 Oct. 23, $11^h\ 35^m$.—Spots 1, 3, 4, 17, and 30 were seen as craters. No. 4 is described as being a more conspicuous crater than 3 or 30, but not so conspicuous as 1 by a good deal. 6½-in. reflector, powers 150 to 320.

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* For some account of the results of the researches in the years 1879 to 1884 see the 'Observatory,' 1883, pp. 85, 112; 1887, pp. 59, 345.

These observations show clearly that up to *quite recently* No. 4 was still in existence *as a crater*. It certainly was so in 1888. Prof. Pickering's observations do not, however, appear sufficient to *prove* that it has now lost its crater form, on account of the peculiar character of the object. According to the writer's observations, the wall of the crater was a low one, and insignificant for the size of the object. The crater was also one of those unsatisfactory features peculiarly prone to assume a hazy ill-defined aspect, so that its visibility *as a crater* would probably depend largely upon a particular kind or angle of illumination. This view is also borne out by the earlier observations. Thus Mr. Gledhill remarked that "this object seems to have *lower* walls than 1, 17, or 3." From the observer's notes in the 'British Association Report' it seldom appears also to have been seen as a crater, and there are frequent references to the hazy nature of the object. From the great map of Schmidt it is absent altogether. These considerations seem to show that the visibility of 4 as a crater is, or was, dependent largely upon a particular kind of illumination, and perhaps other causes, and hence a large number of observations must be obtained under varied conditions before it will be possible to affirm positively that a real change has occurred. On account of the importance of the question, it is to be hoped that selenographers will endeavour to observe this crater whenever opportunity presents itself.

The other two cases of change referred to by Prof. Pickering have not the same importance. No. 31 has rarely been seen as a crater, and as a spot it has always been a faint one and much inferior to No. 30. Thus Birt gives its mean visibility in 1869–71 as ·026 (that of the central spot No. 1 being taken as unity). In 1879–82 it was ·031, and in 1882–84 ·037. The mean visibility of No. 30 in these three periods was ·166, ·306, and ·270, showing it to have been a much more conspicuous object. With regard to No. 7, there is doubt if the crater seen once or twice on the north-east part of the floor was correctly identified with the bright spot No. 7 shown in Birt's map.

With reference to Prof. Pickering's opinion that the stated variability of these craters seems almost impossible of explanation, it should be remarked that the variations found by Birt relate almost entirely to the brightness of the objects *as spots* when seen under a high illumination. It is conceivable that the temporary presence of a little gas or vapour about the site of one of these orifices might materially affect the reflecting power, and so the brightness of the resulting spot, and so cause it to appear variable. It should also be mentioned that many of the spots not seen as craters are bright and conspicuous objects under a high sun, and so were far from being necessarily near the limit of vision of the observers who took part in the observations of 1869–71, as Prof. Pickering seems to assume. The number of such objects has also been greatly increased by recent observations, and more than 70 light spots are now known to exist upon the floor. On one night as many as 60 light spots were seen by the writer, besides about 40 light streaks, and of these only 12 or 15 could be considered as near the limit of vision with the 6½-in. reflector employed.

Several of the new craterlets drawn by Prof. Pickering are identical in position with spots previously observed, but, except in one or two cases, they have not been seen crateriform before, so that their detection forms an important contribution to our knowledge of the details of the floor. It is to be hoped that these interesting researches will be continued. Yours faithfully,

Brighton, 1892, June 16. A. Stanley Williams.

The Greatest Distance of Small Planets.

Gentlemen,—

As Sir Robert Ball has stated, in his popular work 'The Story of the Heavens,' that "in one or two cases the orbits [of the small planets] just reach beyond the path of Jupiter," p. 294, it may be worth while to point out that the aphelion distance both of Thule (No. 279) and Hilda (No. 153) (the former has the greatest mean distance, but a somewhat smaller eccentricity) amounts to 4·6, and that no other known small planet recedes so far from the Sun as this. The perihelion distance of Jupiter is 4·95. The mistake is not corrected in the revised issue of the 'Story of the Heavens.' Yours faithfully,

Blackheath, 1892, June 22. W. T. Lynn.

On the Estimation of Star-magnitudes by Extinction.

Gentlemen,—

Although so many questions were asked at the R.A.S. meeting respecting Captain Abney's paper, it may not be amiss to add one or two more. I do not understand whether his results depended at all on the *area* of the light extinguished, or whether, so long as the area was small, it would make any difference whether the light was concentrated into a point, or if the same total amount was spread out into a larger area. That is to say, in effect, whether the faintest small nebula visible to the eye would have the same total brightness as the faintest star visible. I apprehend this would be the case, but have never seen the subject discussed. Of course if the nebula has any considerable size, this would not be the case, but its visibility would depend entirely on its *intrinsic* brightness. It may be that Captain Abney would say this would be the case with the smallest nebulæ also; it is evident that this point affects the answer to the remark made by Mr. Plummer as to the chromatic correction of object-glasses, for these will probably not be corrected for the violet sensation, and therefore when a star is on the point of becoming extinguished it will be a disk of some size.

Am I right in supposing that Captain Abney's investigations bear equally on the method of limiting apertures?

I do not understand Captain Abney's remark * "that the violet sensation is 175 times more persistent than the red, and probably 50 times more than the green," which seems at variance with his other remark that it is 1500 times as persistent as the red.

In 'Astronomy and Astro-Physics,' p. 303, is given, from the 'Proceedings of the Royal Society,' Captain Abney's curve of extinction of light for the spectrum, each colour having the same luminosity. This curve rises very steeply from near F to near C, beyond which it is nearly level; so agreeing with Captain Abney's statement † that "a little further than C, towards the extreme red, the extinction is the same for every ray"; but at the other end of the spectrum the curve is not horizontal, but rises from wave-length 4500 to 4000; how can this be reconciled with the statement ‡ that from near F to the extreme violet the extinction value remained the same?

Sunderland, 1892, June 17. T. W. Backhouse.

PUBLICATIONS.

Miss Clerke has shown her versatility by writing a book on Homer §. "Since Madame Dacier," says the 'Saturday Review,' Miss Clerke is probably the first lady who has helped to elucidate Homer. She calls hers 'a non-erudite study' and intends it 'for the benefit of the general reading public,' not for experts and specialists .... On the whole Miss Clerke's book is a most pleasant and popular treatment of Homeric Realien." The second chapter is on 'Homeric Astronomy,' and the "astronomical ignorance betrayed" in the 'Iliad' and 'Odyssey' is shown to be an argument against their late origin. "This ignorance has, however, graduations, whence some hints as to the relative age of the two epics may be derived. An attentive study of what they have to tell us about the stars affords some grounds for placing the 'Iliad,' the 'Odyssey,' and the 'Works and Days' in a descending series as to time.

"In the first place, the division of the month into three periods of ten days each is unknown in the 'Iliad,' is barely hinted at in the 'Odyssey,' but is brought into detailed notice in the Hesiodic calendar. Further, the 'turning-points of the sun' are sometimes unmentioned in the 'Iliad,' but serve in the 'Odyssey,' by their position on the horizon, to indicate direction; while the winter solstice figures as a well-marked epoch in the 'Works and Days.' Hesiod, moreover, designates the dog-star (not expressly mentioned in the 'Odyssey') by a name of which the author of the 'Iliad' was

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* 'Monthly Notices,' p. 427.

† 'Observatory,' p. 205. ‡ *Ibid.* p. 204.

§ 'Familiar Studies in Homer,' by Agnes M. Clerke. London: Longmans, 1892.

certainly ignorant. Besides which an additional constellation (Boötes) to those named in the 'Iliad' appears in the 'Odyssey' and the 'Works and Days,' while the title 'Hyperion,' applied substantively to the Sun in the 'Odyssey,' is used only adjectively in the 'Iliad.' Finally, stellar mythology begins with Hesiod; Homer (whether the Iliadic or the Odyssean) takes the names of the stars as he finds them, without seeking to connect them with any sublunary occurrences."

PHOTOGRAPHIC MEASURES OF THE PLEIADES.—The pamphlet of 70 pp. 8vo, which Mr. H. Jacoby contributed to the N.Y. Acad. of Sci., is full of interest. It contains the measures of RUTHERFURD'S photographs of 1872 and 1874. "The Pleiades have been selected for reduction from among a large number of clusters photographed by RUTHERFURD with his 13-inch telescope, because the group offers the best opportunity for judging of the accuracy obtainable by the methods employed." Each plate contains two impressions of the cluster, which were both measured. The method was that of distance and position-angle, from the star 24 *p* (and not *Alcyone*). The orientation is given by trails on each plate terminated by exposures with clock-driving. There are in all 1262 measures. The distances were referred to a glass scale, which was carefully calibrated. Convenient formulæ and tables are given in the paper for the refraction correction. Empirical corrections, though tempting, are not applied. The results are exhibited in a very concise tabular form, and the probable error of the mean of the 20 exposures is about $\pm 0''\cdot03$ in each element. Finally a comparison is made between the photographic places and those resulting from the Yale and Königsberg heliometer measures, with very satisfactory results. As Mr. Jacoby modestly puts it, "We may therefore conclude that the present photographic observations are entitled to be taken into consideration if it be desired to make a study of proper motions, or to form a definitive catalogue of the Pleiades."

IN STARRY REALMS *.—From the preface we find that this is a reprint of various articles that have appeared within the last few years in 'Good Words' the 'Girl's Own Paper,' various newspapers, 'Macmillan's Magazine,' the 'Contemporary Review,' 'Longman's Magazine,' and a presidential address delivered at the Midland Institute, Birmingham, with such alterations as were necessary in gathering them into a consecutive whole. Sir Robert Ball says: "The object of the book is to give the general reader some sketches of specially interesting matters relating to the different heavenly bodies. They may be regarded as supplementary to a treatise on elementary astronomy such as my little volume 'Starland.'"

The author has treated the various subjects in his usual popular and charming manner, and the volume has not the least suspicion of the staleness of a twice-told tale. The illustrations are very good and the printing excellent. The illustrations and examples that the author uses to point his arguments are numerous and extremely good, as, for instance, in giving the size of the Moon in comparison with that of the Earth he shows on page 51 the lunar disc against the map of Europe, and so in giving an idea of the immense size of the lunar volcanoes he gives on page 89 Ptolemæus, Alphonsus, and Arzachel compared with the area of England.

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* 'In Starry Realms.' By Sir Robert S. Ball, D.Sc., LL.D., F.R.S., Lowndean Professor of Astronomy and Geometry in the University of Cambridge, Author of 'Starland' &c. (Isbister and Company.) Price 7*s*. 6*d*.

A chapter near the end of the book gives a most interesting account of the great Krakatoa explosion, that will be read with great interest by those who have not access to the book published by the Royal Society.

NOTES.

Orbit of Neptune's Satellite.—Mr. A. Hall, Jr., finds that observations of this satellite with the 26-inch Washington refractor during 1891 October to 1892 March confirm the reality of the slow motion, nearly proportional to the time, of the orbit plane of the satellite with respect to the orbit of Neptune, to which Mr. Marth called attention ('Monthly Notices,' vol. xlvi.) and which M. Tisserand shows may result from a slight flattening of the planet (Comp. Rend. tome cvii.).

The Variation of Latitude.—It will be a long time before the last word is said on this subject, or even on the present aspect of the question; but Mr. S. C. Chandler has put a more or less definite issue before us in a series of remarkable papers in the 'Astronomical Journal.' He has examined an enormous mass of material—"all the data readily available for the purpose in the published results of the last half-century" is only a part of it—and has presented the whole investigation with such convincing clearness that one could not fail to be convinced, were it not for the recollection of occasions when the apparently invulnerable argument of an advocate has been completely demolished by one equally skilful who follows him on the other side. Not that any such has yet publicly arisen to follow Mr. Chandler; on the contrary, we find such illustrious names as those of Prof. Newcomb and Dr. B. A. Gould among his juniors on the same side. We have perhaps no right to use any parallel which suggests opposition, for none has yet seriously been offered. It is a certain elaboration in the hypothesis he presents, and again a startled surprise at his quiet assertion that our Earth is undergoing secular changes which may be measured in days, which make us look round expectantly for the counsel for the defence. To those who read Mr. Chandler's papers even with considerable and critical attention, it is manifest that the pole of the Earth's figure has recently been revolving round that of rotation in a period of 427 days; but that this velocity has not been maintained for more than a fraction of a century as we look backward. About 1770 the rotation period was less than a year, and it has been increasing since then, while there is reason to think that it was also larger before 1770, attaining a minimum about 1774. The radius of revolution is $0''\cdot22$, so that there is a variation in latitude ranging over nearly $0''\cdot5$. The evidence by which these conclusions are established is so extensive that it is impossible to give even an abstract of it here. We must refer our readers to the original papers in the 'Astronomical Journal.' They are very easy reading, and are certainly worth scrutiny.

We have said that Prof. Newcomb is an adherent of Mr Chandler, but this must be taken to refer to the present 427 day period, and not to its change in historical times. He maintains, indeed, that the period cannot vary. He indicates a possible physical explanation of it in the elasticity of the Earth (*vide* papers in 'Astronomical Journal' and 'Monthly Notices'). Dr. Gould has examined the Cordoba observations, and considers "that Chandler's periodic variation of about fourteen months is clearly manifest in the Cordoba observations," though "the manifestations fluctuate so irregularly during the 103 months considered, that we are compelled to assume that the effect of some other variation is superposed," and "both the period and amplitude of Chandler's variation fluctuated considerably within short intervals." The last remark makes the testimony of somewhat doubtful value; for if more variables are to be introduced, one begins to wonder whether anything definite will ultimately be left.

It cannot be said indeed that the question of variability of latitude is yet one on which a prudent man would cheerfully express an opinion. Too many doctors differ in their diagnosis. Mr. Chandler has shown to the satisfaction of Prof. Newcomb and Dr. Gould that there is a period of 427 days now, but he has shown with the same skill, though *not* to their satisfaction, that this period has not remained constant; while Mr. Loewy and others do not believe even in the 427 day period. Meanwhile a great many series of observations have been ransacked, which must be ultimately for the general advantage.

SOLAR AND LUNAR DIAMETERS.—M. Stroobant has called
attention (though he is by no means the first to do so) to the fact
hat transit observers generally make the solar and lunar diameters
o large. Dr. Auwers recently made some remarks on the
mparison of heliometer measures of the solar diameter with
idian observations. The "occultation diameter" of the Moon
ved from disappearances and reappearances of stars at what
be the real limb (or rather outside it if there is a sensible
atmosphere) has long been recognized as smaller than the
er given by meridian observations. Thus right ascensions
centre deduced by applying the erroneous meridian semi-

diameter positively to the transit of the first limb will differ systematically from those obtained in the second half of the lunation from the second limb; and this will have an effect on the parallactic inequality. M. Stroobant might have added that this error is different in summer and in winter: for it is principally due to diffraction, and diffraction phenomena are modified in summer by the brightness of the background against which the Moon is seen.

TROUVELOT'S OBSERVATIONS OF VENUS AND MERCURY.—M. Trouvelot communicated to the Soc. Astr. de France in February last the results of nearly twenty years' observations of Venus and Mercury. Little of interest is said about the latter, the greater part of the paper dealing with Venus. His conclusions are:—

(1) That Venus is visible to the naked eye in a clear sky up to 10° from the Sun at inferior and 5° at superior conjunction: but at Meudon it was never seen within 20°.

(2) There are vague grey spots which appear near the terminator and are of short duration.

(3) Two of these (1876 Sept. and 1891 February) may have been different apparitions of the same phenomenon.

(4) The brilliant spots observed on the limb are permanent and are very high mountains occupying only the two regions where one ordinarily sees the horns. These mountains are so high as to extend above the dense atmosphere of the planet, and among them are situated the poles of rotation, the axis being inclined some 10° to the orbit.

(5) These mountains are responsible for curious apparent deformations of the terminator, which has sometimes also an S-shaped form.

(6) The limb of Venus is sometimes (rarely) deformed. Reason doubtful.

(7) The phases do not quite obey the geometrical law. Eastern dichotomy occurs before and western after the calculated date.

(8) The definiteness of the terminator is very variable.

(9) Near inferior conjunction the crescent extends beyond the semicircle and may occupy 200° (or even more) of the limb.

(10) When the crescent is very narrow the penumbral border of the terminator is emphasized so that the crescent appears to be made up of two almost separate zones.

(11) The period of rotation is about 24^h.

THE election of a Director of the Cambridge Observatory has not been a smooth and easy matter. The Observatory Management Syndicate's Report was discussed with some warmth in the Arts School and finally referred back to the Syndicate for amendment. After amendment its recommendations stood as follows:—

I. 1. That Sir Robert S. Ball, Lowndean Professor, be appointed Director of the University Observatory; and that this arrangement be terminable at any time on reasonable notice either by the University or by the Professor.

2. That a stipend of £150 a year be allowed to the Director out of the University Chest, and that he be required to reside during 42 weeks in each year.

3. That a sum not exceeding £800 be allowed from the University Chest for alterations in and additions to the Director's house.

II. That H. F. Newall, M.A., of Trinity College, so long as he holds the office of Observer, established under Grace 13 of 22 May, 1890, be entrusted with the sole charge of the Newall Telescope.

Recommendation I. was non-placeted, "on the ground, stated by Dr. Glaisher in the Arts School; that the union of a professorship of mathematics with the directorship of a modern observatory is unfair to mathematics and to astronomy," and "on the ground that by now accepting Mrs. Adams' most generous offer, the University would be enabled to provide for the endowment of a separate director, at a less ultimate cost than that involved in the proposals of the Syndicate."

To the first objection the Professor of Physics replied that an Astronomical Professor without his Observatory appeared to him like a Professor of Physics without his Laboratory. The second objection is of rather a complex nature. Mrs. Adams has offered to endow a John Couch Adams Astronomership; but the endowment is not to be a mere addition to the salary of an already well-paid Professor. She has otherwise no objection to the benefaction forming part of the Director's salary; but Sir Robert Ball's election as Director would delay the acceptance of the offer for more detailed consideration: whereas it might have been accepted at once for part of the salary of an independent Director. Various fly-sheets were issued on the whole question before the Vote on June 9. Ultimately Sir Robert Ball was elected by 54 to 38. We sincerely hope that his future career as Director will be unruffled by any such slight turbulence as has marked its commencement.

THE CONSTANT OF ABERRATION.—We are glad to see that Mr. G. Comstock, of the Washburn Observatory, has been making trial of M. Loewy's method for determining aberration, with the following modification:—"The essential feature of M. Loewy's method is the introduction of reflecting surfaces in front of the objective of a telescope, by means of which images of different portions of the heavens are simultaneously produced in the focal plane of the objective. By means of the micrometer the apparent distance between the images of two stars thus produced may be measured, and the angular distance between the stars determined from a simple relation involving the measured quantity and the angle included between the reflecting surfaces. It is obvious that great difficulties would attend the determination of this angle, and M. Loewy avoids these difficulties by measuring the distances of two pairs of stars and taking the difference of these distances, thus eliminating the angle between the mirrors. . . . It has seemed to me advantageous to place before the objective *three* reflecting surfaces instead of two, making approximately equal angles among themselves, and to employ successively each pair of surfaces in measuring the distance between two given stars. If the normals to these surfaces all lie in the plane passing through the two stars and the Earth, the mean of the three dihedral angles formed by the surfaces will be exactly 120°; and by taking the mean of the results furnished by the three pairs of surfaces, the distance between a pair of stars may be determined independently of the angles between the mirrors."

Mr. Comstock has paid very careful attention to the refraction question, including the exposure of thermometers. Such attention is not necessary in M. Loewy's unmodified method; but in the longer time required for three observations of a pair of stars instead of one the difference of zenith-distance becomes large. His provisional result for the constant of aberration is

$$20''\cdot494 \pm 0''\cdot017.$$

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61 CYGNI.—Prof. Asaph Hall has discussed the observations of this star with reference to its binary character. Accurate observations of distance and position-angle begin about 1825 and have been continued to the present time. Prof. Hall's own observations with the Washington 26-inch extend from 1879 to 1891. "The relative motion is so nearly rectilinear that doubt has frequently been expressed as to the physical connection of these stars and it has been assumed that they will gradually separate and hereafter follow widely different paths through space. . . . It will be seen that the result (of this discussion) is in favour of the physical connection of these stars, but the empirical formulæ employed give but little idea of the period of revolution, except that it is long."

COMET NOTES.—Swift's Comet is now becoming much fainter; at the end of July its brightness will be one ninth of that at discovery. Dr. Berberich has published a continuation of his ephemeris, which is for Berlin Midnight:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
July 6....	0	51	36	47	48	July 26 ..	1	3	37	51	13
10....		55	20	48	36	30 ..		3	56	51	43
14....		58	26	49	21	Aug. 3 ..		3	31	52	8
18....	1	0	52	50	2	7 ..		2	24	52	29
22....		2	36	50	39						

The brightness of Denning's Comet remains nearly stationary at about 0·70. Dr. Schorr continues his ephemeris (for Berlin Midnight):—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
July 6....	5	31	23	39	39	July 26 ..	5	57	6	34	28
10....		37	8	38	37	30 ..	6	1	21	33	25
14....		42	34	37	35	Aug. 3 ..		5	19	32	21
18....		47	43	36	33	7 ..		9	3	31	16
22....		52	33	35	30						

The comet was observed at Hamburg on May 23, its place on that day agreeing closely with Dr. Schorr's ephemeris.

Winnecke's Comet is now bright, being only 11 million miles from the Earth on July 9; it is, however, so near the Sun that it will be practically invisible till the end of the month. The following ephemeris (for Berlin Midnight) is by Dr. Haerdtl:—

	R.A.			Decl.			R.A.			S. Decl.	
	h	m	s	°	′		h	m	s	°	′
July 6..	7	18	44	20	55 N.	July 26 ..	4	17	33	17	5
10..	6	26	44	10	50	30 ..		1	30	20	12
14..	5	41	25	0	58 N.	Aug. 3 ..	3	48	34	22	33
18..		5	31	7	0 S.	7 ..		37	37	24	26
22..	4	38	14	12	52						

On June 15 a telegram was received from Mr. Moss at Muravera, stating "Bright comet near μ Ursæ Majoris." This was probably Winnecke's Comet, which was within a degree of this star on the night of June 14. A. C. D. C.

OPPOSITION OF MARS.—The U.S. Naval Observatory, in a special circular, calls attention to the favourable opposition in August of this year, and earnestly requests the cooperation of "all observatories having the necessary equipment, and especially all those in the Southern Hemisphere," in a special programme of observations. The use of a reversion-prism is specially mentioned. The observations should begin on June 20 and continue until September 23, and this period is subdivided into three. For the first period the comparison stars are O.A.S. 20970, η Capricorni, 27 Capricorni, ϕ Capricorni, Lacaille 8851, 41 Capricorni, DM. $-20°$, 6923, Lalande 42700. Reductions of all observations made in accordance with the circular will be undertaken at the U.S. Naval Observatory.

PRELIMINARY ADDRESS OF THE GENERAL COMMITTEE OF THE WORLD'S CONGRESS AUXILIARY ON MATHEMATICS AND ASTRONOMY. (GEORGE W. HOUGH, LL.D., *Chairman.*)—The World's Congress Auxiliary is an Organization maintained by the World's Columbian Exposition, and approved by the Government of the United States, for the purpose of organizing a series of Congresses or Conventions to be held during the progress of the Exposition in 1893, and which will bring together the leading scholars of the world for the mutual interchange of ideas on topics bearing on human progress.

A Scientific Congress to present and consider investigations in its special lines of research from all parts of the world, cannot fail to exert an important influence on the progress of scientific development. The personal interchange of views in regard to methods of observation and investigation will undoubtedly be productive of mutual benefit to the members of the Congress, as well as of lasting value to science.

The General Committee on Mathematics and Astronomy presents this Preliminary Address, cordially inviting the coöperation of all persons and societies interested in this department of physical science.

As the matter assigned to this Committee covers a large field in Physical Science, it has been thought advisable to arrange the subjects to be considered under the following Chapters and Sections, in which, in consideration of its recent development and growing importance, Astro-Physics has been assigned a separate Chapter from other branches of general Astronomy.

The following are some of the topics suggested for consideration under the several Chapters :—

Chapter I.—PURE MATHEMATICS.
  Section a. History and Bibliography.
  Section b. Arithmetic and Theory of Numbers.
  Section c. Analysis.
  Section d. Geometry.
  Section e. Analytical Mechanics.
  Section f. Mathematical Physics.

Chapter II.—ASTRONOMY.
  Section a. History of Astronomy.
  Section b. Astronomical Instruments.
  Section c. Methods of Observation.
  Section d. Physical Astronomy.
  Section e. Observatory Buildings.

Chapter III.—ASTRO-PHYSICS.
  Section a. Spectrum Analysis.
  Section b. Astronomical Photography.
  Section c. Stellar Photometry.

The object of this Preliminary Address is simply to bring the subject of the Congress to the notice of the scientific men of the world for advice and suggestions as to the general conduct of the convention, and in particular as to the scientific questions to be discussed. Recommendations of themes to be discussed and of persons to present them are especially solicited from the members of the Advisory Council of the Astronomical Congress. The Advisory Councils constitute the non-resident branches of the Auxiliary Committees. Additions to these Councils will be made from time to time. Communications may be addressed to the Chairman of the General Committee, or to the Chairman of the proper Special Committee.

It is expected that men eminent is special lines of research will be invited to furnish papers on the leading topics under consideration. The suggestions and recommendations invited will be used in the formation of the programme for the Congress.

The Chairmen of the Special Committees of the several Chapters under the charge of the General Committee are as follows:—

On Pure Mathematics:
Prof. E. H. Moore, Chicago University, Chicago, Ill.
On Astronomy:
Prof. G. W. Hough, Dearborn Observatory, North-western University, Evanston, Ill.
On Astro-Physics.
Prof. Geo. E. Hale, Kenwood Astro-Physical Observatory, Chicago, Ill.

George W. Hough, *Chairman.*
Elias Colbert, *Vice-Chairman.*
E. H. Moore,
George E. Hale,
G. A. Douglass,
Malcolm McNeill,
R. W. Pike.
Geo. C. Comstock,
W. W. Payne.
Committee of the World's Congress Auxiliary on a Congress of Mathematicians and Astronomers.

Hyperion.—Mr. W. S. Eichelberger, of Wesleyan University, U.S.A., is engaged in a study of the problem of the perturbation of Hyperion by Titan. His first paper (Ast. Journ. 259–60) is devoted to obtaining more accurate elements of Hyperion from an exhaustive discussion.

Parallax of δ Herculis.—Dembowski's 'Double Star Observations,' vol. ii., contain 68 observations of this double star, spread over a period of 16 years. Mr. F. P. Leavenworth has made a series of measures of the same star and obtained a parallax of $+0''\cdot050 \pm 0''\cdot014$, and on reducing Dembowski's observations finds $+0''\cdot030 \pm 0''\cdot015$.

Professor Oudemans wishes us to make the following corrections to the account he gave of the triangulation of Java:—

P. 212 in the note, *for* "the platinum iridium method at 27°" *read* "the platinum iridium meter no. 27."

P. 214, line 10, *for* "on a pentagon" *read* "or a pentagon."

P. 215, line 14, *after* "determined" *add* "by Mr. H. T. Soeters."

We regret to announce the sudden death of Admiral Mouchez, director of the Paris Observatory, on Saturday, June 25. He was on a visit to his country-house at Wissous, near Antony, but on Friday afternoon was busy at the Observatory. On Friday night he was seized with sudden illness, which proved fatal, in spite of the best medical skill, in a few hours. We hope to give next month some particulars of his work, particularly with reference to the International Chart of the Heavens, which owes so much to his efforts.

A Reuter telegram from Vizagapatam, Madras, states that Mr. Narasinga Row, the well-known native astronomer, died there on Saturday, 18th June.

Erratum.—No. 189, p. 268, line 8, *for* "observations" *read* "observatories."

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 191. AUGUST. 1892.

ADMIRAL MOUCHEZ,

Late Director of the Paris Observatory.

Rear-Admiral Ernest Amédée Barthélémy Mouchez, late Director of the Paris Observatory, died suddenly, June 29, 1892, in the fulness of strength and intellect.

He was born in Madrid, August 24, 1821, began his classical studies at the *Lycée St. Louis* in Paris, continued them at Versailles, and at the age of sixteen entered the Naval School at Brest. In the navy he found his true vocation; he was born a sailor and remained a sailor his whole life. The thoroughness of his learning, his earnestness in all his doings, his great dignity of character, the devotion of all his faculties to the service of his beloved France won for him the highest honours.

He was gazetted midshipman in 1839, lieutenant in 1845, captain in 1861. In the Franco-Prussian war he defended Havre admirably. Various scientific expeditions were intrusted to him. His astronomical mission for the observation of the transit of Venus in the island of Saint-Paul was distinguished not only by his scientific knowledge but also by very able organization. On his return he was promoted Commander of the Legion of Honour, was elected a member of the Academy of Sciences, and on the 27th of June, 1878, he was appointed Director of the National Paris Observatory, and the rank of rear-admiral was conferred upon him.

On one occasion (while he was still a subaltern) his commanding officer, M. de Roquemaurel, presented him to the *Ministre de la Marine* with these words: "As for Mouchez, well I need not praise him any longer, his works will say enough for him."

The ardour, the disinterested energy, the highmindedness that characterized his long naval career mark also his fourteen years' administration of the Paris Observatory, from June 27, 1878, to June 25, 1892.

Through his powerful influence the Observatory increased in importance, in beauty, in size. He established two schools of practical astronomy, endowed the Observatory with an interesting museum, contributed to erect an astronomical summer residence on the *Pic du Midi*; he gave a fresh impulse to the various branches of Astronomy. The reobservation of the stars catalogued in Lalande's 'Histoire Céleste,' begun in 1855, has progressed rapidly, and two volumes of the Great Catalogue of Paris, the result of this work, appeared in 1887; two others were published in 1889. Twenty-one volumes of the Paris Observatory Annals have been published; a monthly periodical, 'Le Bulletin astronomique,' was founded by Admiral Mouchez; numerous series of old observations were reduced and published; the library was enriched, the old instruments were transformed and improved, new ones were constructed, such as the two *équatoriaux coudés* and the photographic refractor. He imparted to his devoted staff and fostered in them his own scientific ardour. Useful and interesting researches were achieved, continued, or begun. The errors of the declination circles were investigated and determined; Chacornac's ecliptic charts were continued; the constants for refraction and aberration, as well as the latitude of Paris, underwent a new determination; a new catalogue of all the nebulæ of the northern hemisphere was begun; the sun's spectrum became a subject of special care; a new service was created for spectral analysis and another one for the photographic reproduction of celestial bodies.

This seafaring man, who not long before had determined the longitude and latitude of so many terrestrial places, who so patiently and perseveringly had surveyed the coasts of North Africa and South America, fathomed at once the importance of the magnificent results obtained by his faithful fellow labourers, and, in consequence, he conceived the plan of a vast enterprise at which the astronomers all over the world should be invited to work in a spirit of concord and fraternity. It was not long before his great conception assumed a practical form. Under the auspices of the Academy of Sciences, and upheld by the French Government, who honour and encourage all that is noble and great, Admiral Mouchez convoked foreign astronomers to take part in the Astrophotographic Congress. Thrice, in 1887, in 1889, in 1891, the great and modest *savant* had the signal honour of presiding over a most illustrious company assembled at the Paris Observatory. The photographic Chart, in the work of which eighteen observatories are now actively and earnestly engaged, will be a faithful image of the heavens at the end of the nineteenth century and will keep alive, for many and many a generation, the memory of a man whose eminent qualities drew together the representatives of various nations and united them with himself in the accomplishment of this great work.

For six years Admiral Mouchez's mind was constantly bent on the Chart of the Heavens and was occupied with the preparations for the great international catalogue voted by the Congress. He appealed to his Government and funds were voted for constructing a measuring-apparatus and for establishing a micrometrical service which he opened in February, 1892. A pavilion appropriated for this work is being built close by the photographic instrument, and it was with a child-like pleasure that M. Mouchez watched the progress of the new construction.

It was not granted to him to see the achievement of his great ideal.

He had not been quite well for some days; on the 24th of June, whether feeling the need of a well-deserved rest, or with some mysterious prevision of his approaching end, he convoked MM. Davanne, Loewy, Wolf, Paul and Prosper Henry and read out the programme he had prepared on various subjects under discussion, requesting them to share with him the responsibility of the solution.

Having thus made, as it were, his scientific will, having confided to other hands the great work he had begun, he returned to his beautiful country seat at Wissous. On arriving there he declined all nourishment and laid down to rest, and before another dawn he had peacefully passed away, surrounded by his loving and devoted family, whose pleasure it was ever to please him.

Such was the life and such was the death of this noble and great man, whom it has been my valued privilege to have known and to have served. DOROTHEA KLUMPKE.

BRITISH ASTRONOMICAL ASSOCIATION.

THE last ordinary Meeting of the second session was held in Barnard's Inn Hall, Holborn, on June 29, the President, Capt. W. Noble, F.R.A.S., in the Chair. Five new members were elected, and the names of seven candidates for election were proposed.

The Secretary, Mr. Cottam, announced the gift to the Association by Miss Stevens of a set of lantern-slides of the great spot-group of February last, and the receipt of a framed sheet of drawings of Jupiter made by members of that Section.

Mr. Cottam read a paper by Mr. Calver on "Reflecting Telescopes." Mr. Calver claimed that there was no special difficulty in silvering mirrors, and that real flexure was not observed in good mirrors. Mr. Holmes replied that he had not intended any attack upon reflectors, but he had observed cases of flexure. Mr. Green stated that he had a speculum silvered ten years ago by With which was still as good as ever.

Mr. Townsend read a paper on "Further Equatoreal Spots in 1892," a small group having been noticed near the equator on May 29. In answer to Mr. Freeman he said that the group showed a slight northerly drift. Mr. Maunder remarked that the appearance of these equatoreal spots seemed to point to an early maximum.

Mr. Cottam read a "Note on the Lunar Eclipse of May 11, 1892," by the Rev. T. Cunningham Porter, which elicited some discussion. Mr. Cottam also read papers from Mr. Gore on the "Mass and Brightness of δ Herculis," and from Mr. Eddie on "Observations of Swift's Comet." Messrs. Wesley, Freeman, and Maunder commented on the appearances presented by the tail of this object.

Mr. Maunder announced the formation of the Mars Section, and invited drawings of the Moon made with very low magnifying-powers, a suggestion originally due to Mr. Marth, who explained the circumstances which had led him to make it, and who commented on an anecdote recently published by Prof. Holden with regard to the discovery of Neptune. A long discussion followed, in which several Members joined, on the best methods for drawing the planet Mars.

Messrs. Tyson Crawford, Adams, and Petrie were nominated as Scrutineers of the ballot for the election of the new Council.

The Meeting adjourned at 6.45.

Cometary Notes.

The Comet of 1689.—This object is one of those comets, of which several instances are well known, where from a variety of circumstances the orbit remains indeterminate, nothwithstanding the expenditure of great labour and care on the part of various computers, and is one which still offers a problem to the solution of the curious. The circumstances which have contributed to make the elements uncertain in this particular case are due to the fact that the situation of the orbit was such as to confine the apparent path of the comet to the Southern Hemisphere, where, at that date, the telescopic means were the most inadequate, the shortness of the time during which the comet remained visible, and the conflicting accounts from different observers.

The comet was first seen in the beginning of December 1689, and with the exception of one week during which it was obscured by the brilliancy of the Moon, was seen at Malacca up to the 23rd inst., while at Pondicherry the tail was seen in the first days of the following year "sans pouvoir distinguer la tête qui s'étoit dissipée entièrement à notre égard." It passed in a southerly direction through the constellation of Lupus to Centaur, and in fact very nearly to the star α Centauri. Orbits have been computed from very inadequate details by Pingré, E. Vogel, and Peirce; and a characteristic feature of all the orbits derived, which differ in other important respects, is the small perihelion distance, and this fact coupled with the general description of the appearance of the comet, resembling that given of the Southern Comets of 1880 and 1882, has suggested the possibility of representing the path of the comet by the elements of one or other of that well-known group.

Recently, Dr. Holetschek has attempted to derive by an exhaustive survey of the entire circumstances a more secure orbit from the same materials that these astronomers have employed, but variously interpreted, especially induced to undertake the task by the mention of a fact in the description, which previous computers seemed to have overlooked, or at least failed to satisfy. At the end of the account given by PP. de Rèze and Comille, the observers at Malacca, this note is added: "La plus grande viteſse de son mouvement, fut du 14 de Décembre au quinzième, d'un peu plus de 3 degréz." If these words mean anything, they would imply that the observers had made careful observations from which the daily motion of the comet (confined, as it was, almost entirely to latitude) could be derived; and if this remark is trustworthy, a tolerably satisfactory criterion is provided to test the accuracy of the resulting orbit.

It may not be uninteresting to some of the readers of the 'Observatory,' little versed in this kind of literature, and accustomed to the accurate observations of to-day, to exhibit a specimen of the observations out of which the diligence and patience of calculators have constructed the orbits given in the earlier portions of our cometary catalogues, though it must be admitted that the comet of 1689 is a bad case. We take as an illustration the observation which connects the comet with α Centauri, one of the least uncertain of the positions given of this comet. P. Richaud, observing at Pondicherry, states that on December 21 the comet was distant from the foot of Centaur about a degree, while the observers at Malacca state that on the 23rd it appeared almost touching the northern and western portion of the foot of Centaur. Here, it seems impossible to decide whether the foot of Centaur implies the bright star (α) which defines the foot, or the outline of the figure as drawn upon some map at that time in vogue. Dr. Holetschek considers the second hypothesis without any very satisfactory result; and as we do not know the map used on the occasion, the vagueness is irritating. It might too be suggested that though the word "north" does not admit apparently of the slightest ambiguity, yet "north" might refer to the zenith, and not to the Pole, while the appreciation of a degree on the sky near the horizon would be very different to different observers. But if one is allowed to alter the text the number of combinations that can be made is legion. A greater difficulty arises on other days in identifying the stars to which the comet is referred, and the impression on the writer's mind after a careful study of the facts on which Dr. Holetschek has brought much ingenuity to bear is, that some of the difficulties have been introduced from the observers themselves subsequently mistaking and confounding the stars with which they compared the comet on earlier occasions.

With these uncertainties in the place of the comet at a particular moment, it will not be surprising that corresponding deviations exist in the orbits which have been computed; and omitting for the moment the dozen or so that Dr. Holetschek has deduced on various hypotheses, it is not a little curious that approximately the same path results, for a short distance at least, from elements so different. But of course with the small perihelion distance assigned, the change in the true anomaly, three weeks after perihelion passage, is very small. Below are given the proposed orbits of the comet, and also Dr. Kreutz's elements for that of 1882 II., referred to the equinox of 1690.

	Pingré.	E. Vogel.	Peirce.	1882 II.
T......	Dec. 1·6285 Paris.	Nov. 29·2 G.M.T.	Dec. 2·14 G.M.T.	
$\pi - \Omega$	60° 0′	180° 44′	73° 2′	69° 35′
Ω ...	323 45	90 25	344 18	343 20
i	110 43	120 55	149 35	141 58
log q.	8·2276	8·2772	8·0128	7·8894

None of these orbits satisfy the test proposed, that the daily motion attained a maximum of 3° between Dec. 14 and 15. To Pingré's orbit Dr. Holetschek is inclined to yield the prize for the best representation of the facts, and does not admit that later investigations have improved on his work; and we think he is fully justified in his opinion. The daily motion in latitude is never, it is true, much greater than 2°, but it diminishes noticeably after the 15th, and the computed path of the comet lies near α Centauri on Dec. 22, while Vogel's orbit carries the comet much too far south, giving it a position on Dec. 22 about 8° S. of α Centauri. A consequence of this rapid southerly motion is that the maximum alteration in Southern latitude is not reached till long after the 14th. The path computed from Peirce's elements tends steadily to the South at an increasing rate after the 14th, on which day it is moving some $2\frac{1}{2}$°, and on the 22nd reaches a maximum of 3°. The elements of the comet of 1882 II. can be made to follow the reported path with tolerable closeness if the date of perihelion passage be assumed about Dec. 1 or 2. Then it will be seen that the elements very closely resemble those of Peirce, and have much the same failing. The tendency of the southern latitude to increase does not pause till about Dec. 30, and with no value of T is it possible to bring about a closer approach to α Centauri than some $3\frac{1}{2}$°; for these reasons Dr. Holetschek declines to speak with any certainty as to a connection between the comet of 1689 and those of the 1843–80–92 group.

Dr. Holetschek's results viewed in the light of this particular criterion cannot be considered entirely satisfactory. After a most careful selection of the most probable stars referred to by the observers in the somewhat inconspicuous constellation of Lupus, he cannot obtain a parabola satisfying all requirements. Neither will probable variations among the stars, nor alterations of M (the ratio of the curtate distances of the comet from the Earth at the extreme observations), lead to a result much better than that of Pingré. The orbit with which he appears best satisfied is the following:—

$$T = 1689,\ \text{Nov. } 30,\ 1654,\ \text{P.M.T.}$$

$$\left.\begin{array}{rl} \pi - \Omega = & 78^\circ\ 10'\ 39'' \\ \Omega = & 279\ \ 24\ \ 28 \\ i = & 63\ \ 11\ \ 30 \end{array}\right\}\text{M. eq. } 1690{\cdot}0$$

$$\text{loq. } q = 8{\cdot}80909.$$

This orbit gives a diminishing motion in latitude after Dec. 14, but it never attains during the period of the comet's visibility a daily motion of 3°, and the conviction is reluctantly forced on Dr. Holetschek, that the observer was misled by inaccurate delineations upon an inaccurate chart *. In his efforts to seek a parabola that would satisfy the conditions Dr. Holetschek has considered the possibility of substituting an orbit in which the perihelion distance is enormously increased. To represent the path of the comet by such an orbit is to some extent possible, but the result is excluded by the fact that, on the one hand, the motion of the comet would be such as to have made it visible in the Northern Hemisphere earlier in the year, and with a brilliancy comparable to that which it exhibited in the Southern Hemisphere, and, on the other hand, the description of the formation of the tail is incompatible with a considerable distance from the Sun. Into the discussion of the behaviour of the tail we cannot here follow our author, although this portion of the work is not the least interesting. This tail is described as curved, an important fact in assisting a judgment of the position of the node in orbits so doubtful as this confessedly is. But the uncertainty in the position of the nucleus introduces similar uncertainty in tracing the direction of the tail, and the deductions are correspondingly insecure. We feel that the utmost has been done to remove the halo of mystery that surrounds this comet, and that it still remains to some extent an enigma, like that of D'Angos' and some other comets which have taxed the ingenuity of computers and baffled complete explanation.

Encke's Comet.—"La théorie du mouvement de la comète d'Encke laisse encore beaucoup à desirer." These words, with which Dr. Backlund prefaces his last communication on the subject of this comet, are not cheerful reading, and may create an unfavourable impression as to the thoroughness of the particular work which Encke commenced, Von Asten continued, and which has

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* "Wahrscheinlich hat man diese verhängnissvollen drei Grade durch ungenaues Einzeichnen in eine ungenaue Sternkarte gefunden."

been ably carried on by the hands of Dr. Backlund himself. But it is to be presumed that the words are used relatively, and refer rather to the complete elucidation of small residuals whose full and entire explanation may prove the most interesting and instructive part of the theory, but while they remain unsolved they constitute a blot on a masterly piece of analysis.

The removal of this blot involves the re-computation of the perturbations of the comet from 1819, a work of enormous labour, and one that would test severely the most energetic computer, even if it could be accomplished at all single-handed. That no one computer would willingly undertake the task can be readily believed, and therefore we congratulate Dr. Backlund on the possession of pecuniary assistance, which, private benefaction having placed in his hands, permits him to call in the aid of efficient collaborateurs, thus assuring to astronomers the complete examination of the motion of this comet. The series of publications which Dr. Backlund will publish as the research progresses, commences with a table for the ready compution of the excentric anomaly and radius vector. The well-known equation

$$M = E - e \sin E,$$

which looks so beautifully simple as an expression, but whose arithmetical computation has vexed the soul of so many calculators, and which has been the subject of much (futile?) elaboration by many mathematicians, will, in the case of Encke's comet, at least, give no further trouble, for Dr. Backlund, assisted by the fact that the excentricity of the comet does not greatly vary, has given a table of the excentric anomaly, with the mean as argument for a particular excentricity ($\phi = 57^\circ\ 56'$), and subsidiary tables furnish the correction to the excentric anomaly for increments of that excentricity. No doubt such a table will be of the greatest service in the subsequent computations, but Dr. Backlund has still before him a problem of great length and difficulty, but one, we do not doubt, with which he will effectually cope and lead ultimately to a successful issue.

W. E. Plummer.

[To be continued.]

Selenographical Notes.

Dr. Weinek's Enlargements of Lunar Photographs taken at the Lick Observatory.—Since the erection of the giant refractor on Mt. Hamilton, and its employment in the production of the beautiful pictures of the Moon which are now familiar to all who are interested in astronomical photography, selenography may be said to have taken a new departure, the ultimate results of which will probably tend to modify the commonly received notions as to the character and condition of a great portion of the surface of our satellite, and will furnish, it may be, a clue to a feasible explanation of the causes which have operated in producing many of those details of structure which have hitherto hopelessly puzzled all who have attempted to account for them.

The labours of De la Rue, Rutherfurd, and others in the field of Lunar photography, though of extreme value as affording accurate representations of the grosser details of the visible surface, did but little to extend our knowledge of the more faint and delicate features, such as rills, crater-cones, and other difficult objects, visible only in large telescopes under the best atmospheric conditions, and failed to a great extent in giving a satisfactory picture of the level areas, called "seas," as they really appear under moderately high powers. These shortcomings, however, were of course not due to the inferiority of the work, but to the object which it was sought to accomplish, viz. to produce a good average result. In this respect, the photographs of De la Rue and Rutherfurd have not been surpassed, if equalled, by any subsequent attempts.

Since the commencement of the year 1890, the more perfect of the Lunar negatives taken with the 36-in. telescope at the Lick Observatory have been forwarded by Prof. Holden to Dr. Weinek, of Prague, to furnish, under a special method of reproduction and enlargement, subjects of careful study. Except in the case of Archimedes, none of these reproductions have yet been published and will possibly not be made public for some months to come. Dr. Weinek, however, has been good enough to send me *heliogravures* of two most remarkable examples of his skill and assiduity (Petavius and Vendelinus), with permission to refer to the very noteworthy features brought to light. Dealing first with the Petavius reproduction, which is enlarged 20 times from the original negative, taken 1890, August 31, 14^h 27^m P.S.T., when the west wall of the formation was on the evening terminator, and is on such a scale as to show the floor about 4½ in. in length from north to south, it is at once apparent that we have before us a picture of this walled-plain, which, as regards the interior surface and that beyond the border, displays features which have never before been observed or even suspected. The great rill with its raised flanks and the central mountains are, of course, the most prominent features, but one was not prepared to find no inconsiderable part of the illuminated area of the interior occupied by a number of sinuous little rills of surpassing delicacy, winding like rivulets among the low rounded elevations and hummocky swellings of which the floor seems to consist. Though in many instances the course of these rills appears to be determined by the configuration of the surface, in some few cases they evidently traverse the higher ground, crossing surface swellings much in the same manner as the larger and well-known examples of these features elsewhere. As a rule, however, they are much more undulating, exhibit many more bifurcations, and in more than one instance suddenly divide, enclosing oval or circular areas, and then reunite. A very noticeable feature in connection with them is their frequent association with small shallow pits which often, but not always, mark their termination. In two or three cases these rills were traced running down the bright western flank of the great cleft and meandering among the mountain gaps and passes of the complex western wall. Those in the former situation appear to be of extreme delicacy and were only seen with difficulty on the enlargement. Beyond the border on the east, especially north of Wrottesley, the surface displays a very similar structure to that within the ring of Petavius, and there are here and there faint indications of the presence of small shallow winding valleys, which perhaps, if they were as well defined as those which render the floor so remarkable, would assume a similar appearance. Altogether the drawing strongly impresses one with the idea that the rounded heights, the rills, and the surface generally of the floor are due to the action of fluid in motion. It reminds one of the appearance presented by a bank of mud or semi-consolidated sand on the seashore, recently left by an ebbing tide which has carved on its surface innumerable little hills and valleys and winding runlets, representing the miniature clefts and their bifurcations.

The reproduction of the Vendelinus negative is even more noteworthy than that of Petavius. It bears the same date and has also been enlarged 20 times. Only about one-fourth of the area of the floor is in shadow, viz. a strip on the eastern side. The formation is shown under a lower angle of illumination than in a sketch of my own recently published in the Memoirs of the British Astronomical Association, the complex ring C and the depressions west of the wall having passed out of view. It exhibits the surface of the interior as including a large number of delicate forked rills of a similar type to those in Petavius. Here, however, they are still more like little winding rivulets, increasing apparently in width and depth as they are joined in their course by their tributaries, if such a term may be applied to the forks. Though the floor appears to be less hummocky than that of Petavius, it is far from being an even surface. The rills seem in many instances to bear a distinct relation to its inequalities, a circumstance well exemplified in the case of one of the most prominent of them, two branches of which, originating on the eastern flank of the ring-plain B, on the S. side of Vendelinus, increase in dimensions as they proceed towards the east, join, and, still further widening, are ultimately obscured by the shadow of the border. One of the branches has a very faint companion running closely parallel to it for some distance and coalescing with it at the junction with the other. A noteworthy peculiarity in the minute detail revealed in this drawing is the occurrence of oval and subcircular rings, some of them being considerably larger than the crater D at the northern end of the interior. These are mere scars on the surface, consisting of very low ridges bounded without and within by a shallow trench or vallum, which renders them comparatively conspicuous by its darker tone. In some cases this trench is as sharply defined as the *quasi* rills and appears to resemble them in every way except in their serpentine windings. It is curious in connection with this reproduction, which displays the details of the floor with such marvellous sharpness, that many craters that may be described as easy objects in telescopes of moderate size and which undoubtedly exist in the positions shown in drawings, published and unpublished, are scarcely traceable. This is especially the case with those in the neighbourhood of D, which, at a phase not differing very materially from that of the negative in question, appear as bright little rings filled with black shadow. Two larger depressions on the south-western side of the floor, though very prominent at sunset, are also very inconspicuous in the *photogravure.* The curious banks which abut on the eastern border of the formation C are beautifully shown, the more southerly of them being traversed by a cleft which also cuts across the western wall of Vendelinus. The crater drawn by Schmidt and others on the outer slope of the south-eastern border is shown to have a close companion on the west. The region beyond Vendelinus on the east, like the surface of the floor itself, is scored with rill-like markings and shallow valleys, though they are less prominently displayed.

The remarkable work done by Dr. Weinek in connection with the Lick negatives demanded an expenditure of time, skill, and patience which has rarely, if ever, been so ardently devoted to lunar photography. The publication of the *photogravures* with a description of his method of enlargement and reproduction will be anxiously looked for and warmly welcomed by selenographers.

THOS. GWYN ELGER.

Beaumont House, Shakespeare Road,
Bedford, 1892, July 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Lunar Notes.

GENTLEMEN,—

I should like to take up a portion of your space with some remarks supplementary to a paper on the Lick Observatory lunar photographs which I published in the June 'Observatory.' On the evening of June 21, 1892, Prof. E. E. Barnard, of the Lick Observatory, very kindly gave me an opportunity to examine the Moon with the 12-inch telescope and a power of 700 during such intervals as were not used by the 150 or 200 visitors present.

The seeing was very fine but not perfect (Wt. 4). The crater I had found on the crest of the S.W. wall of Clavius *i* was a very easy object and its existence was confirmed by Mr. Barnard and myself. It was seen, however, that the direction of the line through this crater and the one distant to the S.W. one diameter of *i* had not been given quite correctly, but *n'importe.*

In my paper in the June 'Observatory' I said of the double crater between Ball and Sasserides C: "The southern component of Schmidt's double crater does not look like a true crater, but like a mere depression between hills." "Southern" is a misprint for northern. These craters, together with the valley running down the southern inner wall of Ball, were finely seen with the 12-inch. The northern crater appeared to be an object of the same nature as the southern one, but they both looked more like dried lake-beds than volcanic vents. The northern crater had a distinct low northern wall separating it from the valley. If we were to assume the northern crater to have been a lake, we might, perhaps, explain the existence of this valley by the theory that part of the northern bank of the lake once gave way, thus precipitating a volume of water down the south side of Ball which scooped out the valley.

No crater was seen adjoining Purbach A on the S.E., but a large number of crater-pits were seen just south of Copernicus; they averaged smaller than those in the plain between Copernicus and Eratosthenes. No craters were seen on the summits of the central peaks of Copernicus. That half of the floor south of the central peaks was rugged; that north smooth. Although the eastern wall of Copernicus was splendidly seen, none of the terraces which Prof. E. S. Holden and others have described on it were seen. But it was covered with ridges concentric with the ring-plain, and which could not possibly have been terraces, as they cast shadows towards the east, whereas had the wall been terraced shadows could only have been seen under a descending Sun. The craters Neison shows on the inner wall of Copernicus in his special drawing were not noticed, but were not carefully searched for. Evidently they are difficult objects.

As the Moon's phase was only about 30 hours more advanced than in Neison's drawing of "Plato at Sunrise," an attempt was made to see the craters which this drawing shows on the floor of Plato. For this observation a power of 1000 was used to good advantage. Craters were seen with it that could not be recognized as such when the power of 700 was put in. Yet I only noticed four craters, one of which (No. 1) was a conspicuous object, whereas Neison's drawing shows eight. However, Professor W. H. Pickering has found that two of these eight have disappeared. Furthermore I observed in a great hurry, and no head-rest was used, so that as a negative observation this counts for nothing, but simply proves how important it is to use a head-rest in observing, and that delicate objects seen with a small telescope under exceedingly favourable conditions of illumination and definition need not necessarily be *conspicuous* in a large one under less favourable conditions. It also speaks volumes for the excellence of Neison's eyesight. It may be interesting to add in this connection that no fog to check radiations lay in the valleys around Mt. Hamilton that night, and that no one could desire a more beautifully clear day than that which broke over the Santa Clara valley next morning. Evidently the seeing at Mr. Hamilton is to a certain extent independent of the fog.

Much might be said in reference to Prof. W. H. Pickering's paper in the June 'Observatory,' and the light which Schmidt's map throws on it, but it was thought best to send such information to Prof. Pickering direct. However, I will refer to the two most interesting points. In Neison's drawing of "Plato at Sunrise," he shows one of Prof. Pickering's new craters (No. 40) and omits No. 7, which Prof. Pickering can't see. Schmidt's map of Plato shows No. 30 east of No. 3, as Prof. Pickering draws it, instead of north as Neison draws it, and omits No. 31, thus agreeing with Prof. Pickering.

I would like to draw attention to the fact that in Neison's map xvii. two of the ring-plains in Clavius are lettered D and *e*. They contain central peaks which Schmidt maps and which I saw with an eight-inch refractor. But Schmidt letters the ring-plains *d* and C, and transfers the letter *e* to the enclosure contained by the two ridges which abut against Clavius *b* on the east. This conflicting nomenclature is likely to cause confusion.

I will not waste valuable space in discussing here whether Neison maps Fra Mauro B quite correctly, for that question can be best settled by each person for himself by referring to a photograph of the region (as Rutherfurd's of Nov. 29, 1873) in which the Moon is shown at about its mean libration, but will advise those who admire Neison's map and desire to be convinced that there are many errors in it, to compare his representation of the formations immediately surrounding Wilhelm II., or of those bordering the *Mare Nectaris* on the west, or of Pitiscus and neighbourhood with good photographs which show these regions well, or with the Moon itself. Yours truly,

Berkeley, California, 1892, July 4. ROGER SPRAGUE.

Gregory's Comet.

GENTLEMEN,—

It may seem strange that the discovery of a comet should have been the solitary achievement of an observatory; yet such appears to have been the case with that of Langar, in Nottinghamshire, at the end of last century. The second comet of 1792 (so reckoned because it passed its perihelion on the 27th of December in that year) was discovered by the Rev. Edward Gregory, rector of Langar, on the 8th of January, 1793, and by Méchain and Piazzi two days afterwards. Mr. Gregory states (Phil. Trans. vol. lxxxiii.) that he was taking places of Venus when "happening to look towards the north-western part of the hemisphere, I saw a star of a hazy appearance and about the size of a star of the second magnitude in the space between the flexure of the Dragon and the foot of Hercules, larger and brighter than I had before remarked in that part of the heavens." He suspected it to be a comet, but as the evening twilight was not quite gone, could not be certain of it. "When the night had completely come on, it became evident that it was a comet, the coma being of a white light, hazy and ill-defined." After obtaining a meridian sub-polar observation of the comet, Mr. Gregory continued to watch it until 3 o'clock in the morning, when a faint tail was perceptible, about 3½° in length. He afterwards wrote to the Astronomer Royal, Dr. Maskelyne, who obtained a series of observations of the comet at the Royal Observatory from January 18 to February 7. But in the meantime it had been observed, as already mentioned, on the Continent.

The letters to Maskelyne, communicating the discovery and observations of this comet, are, so far as I am aware, all that ever proceeded from the Langar Observatory. Indeed the only other mention of Gregory in connexion with science is an account which he sent to Dr. E. W. Gray, of the earthquake which was felt in the midland counties of England (apparently most strongly in Nottinghamshire) on the night of the 18th November, 1795. Mr. Gregory was at the time at Wollatan, about three miles to the west of Nottingham, where the shocks occurred at more than 20 minutes past 11 and nearly half an hour later than at Bristol and other places in Gloucestershire.

Gregory's observatory is mentioned in Thoroton's 'Antiquities of Nottinghamshire,' vol. i. p. 208, where he says:—"Here are about thirty dwellings in the former [*i. e.* Langar]; one of which, exclusive of Lord Howe's seat, is occupied by the Rev. Mr. Gregory, the present rector; a gentleman whose astronomical pursuits are spoken of with much respect. He has lately erected an observatory near his house, which has order and embellishments without, as well as usefulness within."

But it must be presumed that its duration was not long. The sole discovery made there was, as mentioned above, almost exactly a century ago.

Yours faithfully,

Blackheath, 1892, July 8. W. T. LYNN.

A Turkish View of the 1724 *Eclipse.*

GENTLEMEN,—

Not only does Dr. Stukeley seem to have been the only recorded witness of the total eclipse of 1724 in this country, but his account was generally overlooked till a recent time. Dr. Baily, writing in 1818 about the coming eclipse of 1820, remarks that "the umbra in the total eclipse of May 11th, 1724, appears to have proceeded over great part of England; nevertheless I cannot find any observation of it in this country." The late Astronomer Royal was also at Greenwich many years before he was aware of it. Coley's Almanack for 1724 gives digits eclipsed for the meridian of London 11.40. Leadbetter for the same year remarks "the second will be a great (and almost total) eclipse of the Sun on Monday the 11th of May, beginning 5.39.52; middle 6.36.19; end 7.25.54. Digits eclipsed 11°·45′," and urges "all ingenious gentlemen that are able and fitted with proper instruments to observe the same and to communicate their observations." Dr. Stukeley remarks in his account, "for some time there were invisible streaks of rays, tending to the place of the sun as their centre." Does this refer to the corona?

There is, however, a note about this eclipse in Horace Welby's 'Predictions realized,' 1862, which may be of some interest to readers of the 'Observatory,' viz.:—"In the year 1724 when the scheme of the great solar eclipse, which foretold the precise time of its beginning and ending, was cried about the streets of London, it happened to be heard by a Turkish envoy, then at our court. He at first thought the people distracted for pretending to know so very exactly when the Almighty would totally overshadow the Sun, a circumstance of which the Mussulmans were ignorant. He concluded that God would never reveal so great a secret to infidels, and keep it concealed from the true believers. However, when the eclipse came actually to pass, as had been predicted, Lord Forfar inquired of his Turkish Excellency what he now thought of the English mathematicians. He replied 'they must certainly have obtained their intelligence from the devil; for he was sure that God would never correspond with such a wretched set of unbelievers as the English astronomers.'" From the phrase "when the eclipse came actually to pass" we might infer that it had been seen in London, only Dr. Stukeley states that he believed he was "the only person in England that regretted not the cloudiness of the day," whence we must infer that clouds generally prevailed over the country on this evening, thereby obscuring a phenomenon the like of which would not occur in England again for more than four centuries, viz. until 2151, the intermediate total eclipses being of comparatively very small interest.

Melplash Vicarage, Bridport,
1892, June 7.

Faithfully yours,
S. J. Johnson.

The Floor of Plato.

Gentlemen,—

Unfavourable weather and the low altitude of the Moon have prevented my obtaining any conclusive observations of the crater No. 4 under a low illumination during the last two lunations. But the observations have resulted in the discovery of two new details on the floor of Plato, which it appears advisable to at once put on record. These new objects are (1) a light-streak and (2) a rill or fault.

(1) This streak was first seen on June 7 last between 10^{h} 40^{m} and 11^{h} 20^{m}, with power 150 on my 6½-inch Calver reflector. Definition was fair, but the Moon's altitude low. The new streak extends from spot 14 to spot 16, so that it continues the old, bright, streak E across the floor almost to the N. border. Although not a bright streak, it was yet so evident that it attracted my attention almost at once; and it is curious that it should have been discovered on a rather unfavourable night when many of the old and well-known light streaks could not be discerned at all, although looked for. The central spot No. 1 was seen as a crater, as it generally can be under any illumination if the seeing is pretty good. Spot 4 was decidedly inferior to 1 and not very well defined. I propose to name this new streak Z^2.

(2) Plato was observed on June 3 from 9^{h} 0^{m} to 9^{h} 30^{m} and again from 11^{h} 0^{m} to 11^{h} 20^{m}, with powers 150 and 225. The western part of the floor was still covered by the shadow of the west wall. From the north border nearly N. of the position of spot 16, an extremely fine and delicate black line was observed extending in a S.S.W. direction nearly as far as a line drawn through spots 1 and 4, when it disappeared in the great central spire of shadow. This black line exactly occupied the position of the new streak Z^2, and is without doubt either a delicate rill or else a fault. A somewhat similar feature has been occasionally observed extending across the floor in a direction from N.W. to S.E., but I am not aware of anything having been observed before in the position of the new marking.

Yours faithfully,

W. Brighton, 1892, July 21. A. STANLEY WILLIAMS.

Is the Sun a Variable Star?

GENTLEMEN,—

It has often been suggested that our Sun might be variable in light, though the general opinion is that if any variation exist, it is very slight. Without doubting this opinion, would it not be advisable to have observations to test the question, although the result may prove a negative one? It seems to be very difficult to make measurements of the Sun's own light, but could not the problem be attacked more easily by observing its light as reflected from planets? The minor planets would be especially suitable for the purpose, and probably also some of the satellites. If systematic observations were made of a number of these, any discrepancies arising from irregularities of surface of the planet, or from its not being perfectly circular, would be eliminated. There would be a difficulty about the correction to apply for phase, which might be overcome by always observing the planet at the same apparent distance from the Sun; or if a sufficient number of observations were made, corrections for phase would be obtained. This would itself be a matter of considerable interest, and would have a bearing on the theory of the Gegenschein of the zodiacal light. If observations such as I have indicated were carried out, and if it were found that all the planets became brighter or fainter simultaneously, it would be evidence that the change was in the Sun. If, on the other hand, one planet brightened while another faded, it would be at once shown that one was variable. Any planet that might be found specially variable should be disregarded in taking the average. It is evident that if observations were made as suggested a variation in the Sun amounting to one-tenth of a magnitude could be easily detected, and probably a much smaller variation would be discoverable.

Yours faithfully,

Sunderland, 1892, July 15. T. W. Backhouse.

Copernicus and Mercury.

Gentlemen,—

We are often treated in astronomical books to a melancholy picture of the aged Copernicus sighing on his death-bed at the thought that he had never been able to obtain a sight of the innermost planet Mercury. This is usually ascribed to his unfavourable locality, and to the frequent fogs prevalent in the lower part of the Vistula; though it is well known that Copernicus spent about nine years of his younger life in Italy, and taught astronomy for some time at Rome.

It seems to me that the idea that he never saw Mercury is founded entirely upon a passage in his 'De Revolutionibus Orbium Cœlestium,' lib. v. c. 30, in which he discusses recent observations of the motions of that planet. In the preceding chapter he had referred to the Alexandrian observations of Mercury given in the 'Almagest.' Here he remarks that those who made them were aided by clearer skies than prevailed in his own neighbourhood, as the Nile did not exhale mists similar to those of the Vistula. "Nobis," he adds, "enim rigentiorem plagam inhabitantibus, illam commoditatem natura negavit, ubi tranquillitas aëris rarior, ac insuper ob magnam sphæræ obliquitatem, rarius sinit videre Mercurium."

The last sentence is quoted by Gassend in his life of Copernicus. It by no means implies that Copernicus had never *seen* Mercury even in his native country; but is given as the reason why, in discussing the motions of that planet, he uses observations made at Nürnberg, as a more favourable locality. "Multis," he says, "propterea ambagibus et labore nos torsit hoc sidus, ut ejus errores scrutaremur. Mutuavimus propterea tria loca ex eis, quæ Norimbergæ diligenter sunt observata." The first of these observations was made by Bernhard Walther, the friend of Regiomontanus (Müller) and founder of the first European observatory, on the 9th of September 1491; the second and third by John Schoner on the 9th of January and the 18th of March 1504. That was the year, it will be remembered, in which Walther died, his observations being continued and published some years later by Schoner, who afterwards became Professor of Mathematics at Nürnberg, where he died on the 16th of January, 1547, one month and two days after the birth of Tycho Brahé.

It is well known that the work of Copernicus was not published until just before his death; and the passage in the fifth book which is quoted above is thus referred to by Gassend:—"Mirere, cur nullam heic videas observationem circa Mercurium; sed causam ipse refert ad auras, seu vapores, quos illeic loci Wistula effundit; itemque ad Sphæræ obliquitatem, quæ *rariùs*, inquit, *sinit videre Mercurium*." The notion that Copernicus never saw the planet was probably derived from a misunderstanding of this passage in his life by Gassend; but it is strange that the remark about the fogs of the Vistula should have been attributed to the latter. It seems to me unwarrantable to infer from the expression in Copernicus that he had never seen the planet at all, particularly as we know that part of his life was not spent in the neighbourhood of the Vistula. Yet Arago ('Astronomie Populaire,' lib. xviii. c. 2) transforms the expression into a supposed "remarque chagrine de Copernic qu'il descendrait dans la tombe avant d'avoir jamais découvert la planète." And this "very pretty tale" (as Mr. Waugh calls it in the February number of the 'Journal of the British Astronomical Association') has been repeated in substance in a large number of astronomical books since. The remark about the fogs of the Vistula, it may be added, can only apply to the lower part near which Copernicus was born (at Thorn) and lived (at Frauenburg) after his return from Italy; but for about four years before he went to the latter country he resided at his paternal town of Cracow, near the source of the river in question.

Yours faithfully,
W. T. LYNN.

Blackheath, 1892, May 2.

The Perseids.

GENTLEMEN,—

The investigations of the late Mr. Joseph Kleiber ('Monthly Notices,' March 1892) have fully corroborated the displacement which I observed in the radiant-point of the Perseids, and the question may be regarded as finally disposed of in the affirmative. But the agreement of observation and theory is not perfect, especially as regards the shower at its earlier stages and at its termination, when the displacement appears to be somewhat greater than theory requires.

On Aug. 16, 1877, I observed the Perseid radiant at 60° +59°, but Dr. Kleiber placed the theoretical position at 54° +59°. Further observations are therefore very necessary in order to determine the exact place of the radiant between Aug. 14–22. My radiant at 60° +59°, Aug. 16, may possibly be a little too far east, for on that date in 1884 I saw a Perseid, as bright as Jupiter, which had a slow foreshortened path from 52° +63° to 51° +67°, and this would indicate a radiant at 53° +59° close to the theoretical position.

I believe that Perseids continue to fall until Aug. 22, and in 1884 I suspected the radiant to lie at 77° +56½° on that date, but from very slender evidence. It is to be hoped that the concluding portion of the display in the present year may be attentively watched, especially with regard to the position of the radiant.

Yours faithfully,

Bristol, 1892, July 24. W. F. DENNING.

OBSERVATORIES.

OXFORD.—The continued prosperity of the Oxford University Observatory may best be gathered by the verbatim quotation of one or two paragraphs from Prof. Pritchard's Seventeenth Annual Report, read June 8, 1892:—

"The work upon the International Photographic Chart, new as it is, in this and other Observatories, has necessitated the acquisition of additional instruments of a more or less important character. Among these the most costly and important is a new form of micrometer, adapted to the measurement of photographic plates, of the unusually large size 160 mm. square. The réseaux, supplied through the kindness of Dr. Vogel, have not proved so enduring as I anticipated. One of them is with Dr. Gill, who has executed an elaborate enquiry into the errors of the ruling, and a new one, constructed by Messrs. Gautier of Paris, has been purchased to supply its place. . . . .

"*Astronomical Work.* As anticipated in the last Report, the work connected with stellar parallax is now complete, and I have placed upon the table a MS. containing the results of that research. I need hardly say that it has been a work of unremitting labour, and one which has occupied the strenuous efforts of myself and the Observatory staff, during the last four years. The MS. thus completed consists of, (1) the concise but complete history of all effective researches in stellar parallax up to the present date; (2) the results of the parallax work completed in this Observatory, extending on the whole to some thirty stars; (3) a catalogue of all parallactic determinations effected by other astronomers; and it is hoped that this catalogue will not only exhibit the present condition of the subject, but will assist all future studies in the same direction. . . . .

"It may be gathered from the preceding statements, how much of the success of the Observatory is due to the assistance and experience of the Senior Assistant, Mr. Plummer, who has been likewise ably seconded by Mr. Jenkins."

The concluding paragraph will give all our readers great pleasure:

"The state of my health and other circumstances prevented my being present at the last meeting of the Board, say that the anticipation of the speedy and con mentioned in the last Report, has been fully realise

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Yale College.—Dr. Elkin's report for 1891–2 shows t heliometer is not allowed to lie long idle. In the last opposit of Jupiter, 570 complete measures of the relative position of th satellites were obtained, which are to be reduced, together with a similar series made by Dr. Gill at the Cape. Since January, Dr. Chase has finished the triangulation of the cluster in *Coma Berenices*, and has commenced a series of measures on Algol with a view to detection of parallax and possibly of orbital motion. Dr. Elkin himself has worked hard at the parallaxes of 1st magnitude stars, being ambitious of getting so many observations that the probable error will be reduced to ±0″·01. Up to the present he has only arrived at the figure ±0″·02, which would, however, satisfy most people; and his results are as follows:—

α Tauri	+0″·10	α Leonis	+0″·09
α Aurigæ	+0 ·10	α Boötis	+0 ·02
α Orionis	+0 ·02	α Lyræ	+0 ·09
α Canis Minoris	+0 ·34	α Aquilæ	+0 ·21
β Geminorum	+0 ·06	α Cygni	−0 ·01

Bidston.—Congratulations to Mr. W. E. Plummer on his appointment as Director must almost be accompanied by condolences with Prof. Pritchard on his loss of so excellent an assistant. Since the time of the elder Hartnup, astronomical work at Bidston has rather languished, but we may hope now to see the name of the Observatory again on the active list. We shall be interested to hear Mr. Plummer's first report, which will, no doubt, be more or less of a programme, and may tell us something of the present instrumental equipment or suggest one for the future.

NOTES.

Trinity College, Dublin, Tercentenary.—The Tercentenary Festival of Trinity College, Dublin, was inaugurated on July 5 in a manner well worthy of the occasion.

Notwithstanding the rather unfortunate fact of its falling on a time when the elections were in full swing throughout the kingdom, the only effect seems to have been that certain eminent men have not been able to attend who would otherwise certainly have done so, and an event of great interest did not receive that attention it would in ordinary times have had from the British public.

The opening ceremony was the reception of the various guests and delegates; after this a procession of something like 1200 persons, mostly in academical costume, was formed, and walked to St. Patrick's Cathedral, where the commemoration service was held.

An appropriate sermon was preached by the Dean, who, in the course of his address, touched upon the history of the Irish Universities from the 7th century, and the early connection of Trinity College with the cathedral, where in former times its commemorations were held and its degrees conferred.

A Garden Party at the College and a Ball at the Mansion House concluded the first day.

On the next day honorary degrees were conferred upon a number of distinguished men. Of these we must mention as representing science who received the degree of Doctor of Science, J. Burdon-Sanderson, Oxford; Michael Foster, Cambridge; Sir George Murray Humphry, Cambridge; Julius Kollmann, Basel; Rev. Thomas George Bonney, Cambridge; Rev. William Henry Dallinger, London; Alexander Macalister, Cambridge; M. Richet, Paris; Sir William Turner, Edinburgh; Wilhelm Waldeyer, Berlin; Sir Archibald Geikie, London: these represented anatomy, physiology, botany, zoology, and geology. As representing chemistry and physics, John Hall Gladstone, London; George Downing Liveing, Cambridge; John William Strutt, Baron Rayleigh, Cambridge and London; Joseph John Thomson, Cambridge; Thomas Edward Thorpe, London; William Augustus Tilden, Birmingham, received this degree.

Luigi Cremona, Rome; James Whitbread Lee Glaisher, Cambridge; Paul A. Gordan, Erlangen; and Edward John Routh, Cambridge; also as representing mathematics.

And as representing astronomy, the same honorary degree was conferred upon George H. Darwin, Cambridge; Simon Newcomb, Washington; and Isaac Roberts, Crowborough.

A banquet wound up the day's proceedings, the chair being taken by the Earl of Rosse, supported by His Excellency the Lord Lieutenant, the Lord Chancellor of Ireland, the Lord Primate, the Marquis of Londonderry, Lord Kelvin, Sir Frederick Leighton, the Right Hon. David Plunkett, the Bishop of Oxford, and Dr. Ingram on the right, and on the left by the Provost of Trinity College, General Viscount Wolseley, the Lord Mayor of Dublin, the Lord Archbishop of Dublin, the Marquis of Dufferin and Ava, Professor Lamelogue (Paris), the Bishop of Derry, and the Master of Trinity College, Cambridge; after the usual loyal toasts Dr. Ingram proposed the toast of the evening, "The Universities," and the Lord Bishop of Oxford responded.

On the following day in the Leinster Hall the Right Hon. the Earl of Rosse, K.G., Chancellor of the University, in the chair, received the addresses from the many delegates who had come from all parts of the civilized world for the purpose. We regret that our space does not allow us to give any account of the many addresses that were thus delivered.

The festival was a magnificent success from beginning to end, and the remembrance will always be with those who had the privilege of taking part in it.

Y Cygni.—M. Dunér calls attention to the anomalous variation of this star, and shows cause for thinking that it "consists of two equally large and bright components, which revolve around their common centre of gravity in an *elliptic* orbit, with a period of revolution of $2^d\ 23^h\ 54^m\ 44^s$ . . . . It is therefore to be hoped that European astronomers will devote more attention to this star than they have done hitherto."

Times of minima are :—

	d	h	m		d	h	m
1381 August	8	7	46	1441 November ..	6	5	5
1401 September..	7	6	52	1461 December ..	6	4	12
1421 October	7	5	58				

Colour Vision.—The report of the Royal Society's Committee on Colour Vision, printed in their 'Proceedings,' and for sale at the price of 4*s*., contains matter of great interest to Astronomers, not only from the point of view of their vocation, but from that of their private life.

"After weighing the evidence which they have obtained, the Committee have unanimously agreed upon the following recommendations :—

"1. That the Board of Trade, or some other central authority, should schedule certain employments in the mercantile marine and on railways, the filling of which by persons whose vision is defective either for colour or form, or who are ignorant of the names of colours, would involve danger to life and property.

"2. That the proper testing, both for colour and form, of all candidates for such employments should be compulsory.

"3. That the testing should be entrusted to examiners certificated by the central authority.

"4. That the test for colour-vision should be that of Holmgren, the sets of wools being approved by the central authority before use, especially as to the correctness of the three test colours, and also of the confusion colours. If the test be satisfactorily passed it should be followed by the candidate being required to name without hesitation the colours which are employed as signals or lights, and also white lights.

"5. That the tests for form should be those of Snellen . . . It would probably, in most cases, suffice if half normal vision in each eye were required."

Other resolutions follow. For the details we must refer to the report itself.

BABYLONIAN ASTRONOMY.—Until the recent labours of Fathers Epping and Strassmaier, our knowledge as to the precise acquaintance of the Babylonians with the motions of the heavenly bodies had been of the scantiest. But from this happy union of assyriological and mathematical learning there has resulted an insight into a wonderfully accurate system of empirical astronomy employed by these ancient observers. An account of the chief results so far obtained by the learned Fathers has recently been brought to the knowledge of English readers in a pamphlet by Father Cortie, a reprint of an article in the April number of the 'Month.' With regard to the chronological results the chief is the fact now firmly established that the year 189 of the Seleucidæan era, and 125 of the era of the Arsacidæ, corresponds to −122 of the Julian era. The months of the Seleucidæ were lunar, either of twenty-nine or thirty days, and their years in which they employed intercalary months were luni-solar. The year commenced with the month Nisan, the first of this month for the year 189 S.E. falling on March 25 −122 J.E. Their civil day of twenty-four hours commenced at sunset, while their astronomers reckoned their day from the midnight after the beginning of the civil day. The astronomical day was divided into 360 time degrees. Among the astronomical results are the deciphering of tables which contain a complete method for calculating the true Moon, the time by which the last rising of the Moon before conjunction preceded sunrise, the determination of the new Moon, and of the interval between new Moon and first crescent. They had a remarkable knowledge of the synodical and anomalistic months, and of the mean velocity of the Moon. They also issued calendars for popular use, containing the more remarkable circumstances of a lunar revolution, as also with surprising accuracy the prediction of all eclipses. Some of these calendars have been fully explained by Fathers Epping and Strassmaier. There were also planetary tables, in which were predicted the conjunctions of the planets with the more important zodiacal stars, as also their oppositions, stationary points, and heliacal risings and settings.

TOTAL SOLAR ECLIPSE OF 1893, APRIL 15–16.—The line of central eclipse cuts both coasts of S. America and the western coast of S. Africa (Senegambia). On the east coast of Chili the line passes some 200 miles north of Santiago, and totality will last only 1 minute 5 seconds, and will be soon after sunrise. An observer will be sent to this station from the Lick Observatory to take photographs "of the inner and outer coronas on the general plan which was so successful at the eclipses of January and of December 1889." The Harvard College Observatory has a station in Peru, and will probably observe the eclipse somewhere in South America. Expeditions will be sent from England, by a joint committee of the Royal Astronomical Society, the Royal Society, and the Solar Physics Committee, to the Brazilian coast (in spite of very hopeless reports from Mr. L. Cruls as to the probabilities of fine weather) and also to Senegambia. Photographs will be taken at both stations, both of the form and spectrum of the corona. Recently Prof. H. S. Pritchett has pointed out that perhaps the best S. American station is that of Rosario de la Frontera, in the Province of Salta, a small town on the railway from Buenos Ayres to Jujuy, 600 miles from Buenos Ayres. The meteorological conditions here are far more favourable than at Ceara. Prof. Pritchett writes an interesting article on the subject in 'Astronomy and Astrophysics' for June 1892.

NEW SOUTHERN VARIABLES.—Five new variables have been discovered by Mrs. M. Fleming on the photographs of stellar spectra taken by Prof. W. H. Pickering in Peru. One of them is apparently sixth magnitude or brighter at maximum, the position being R.A. $2^{h}\ 49^{m}\cdot5$, Decl. $-50^{\circ}\ 10'$.

MR. BURNHAM has resigned his position as Astronomer in the Lick Observatory, and has accepted that of Clerk of the United States District and Circuit Courts at Chicago—a very honourable and a very lucrative appointment. It is to be deeply regretted that his services will thus be lost to astronomical science; but he leaves behind him a noble monument "ære perennius" in the shape of a large number of observations of double stars, which will appear as Volume II. of the 'Publications' of the Lick Observatory.

PROF. C. A. YOUNG has demanded a universal spectroscope, which he can use for eye observation or photography, for Sun-spots, or prominences, or stellar work; and Mr. Brashear has made an instrument, described in 'Engineering' for July 22, which seems well calculated to fulfil these requirements.

WE understand that MM. Tisserand and Loewy have been selected by the French Académie des Sciences as two nominated candidates, one of whom will be elected by the Government to succeed Admiral Mouchez in the Directorate of the Paris Observatory. The actual appointment may not be made for some time.

ERRATUM.—No. 190. In the second paragraph of p. 290, line 2, *for* 7·3 inches *read* 9·3 inches.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY

No. 192. SEPTEMBER. 1892.

MEETING OF THE BRITISH ASSOCIATION.

The Sixty-second Annual Meeting of the British Association for the Advancement of Science was held at Edinburgh on August 3 (and subsequent days), when the Report for the year 1891–2 was read and accepted. In the evening the retiring President, Dr. Huggins, introduced his successor, Sir Archibald Geikie, who gave a most interesting address—in the main, as might be expected, geological. This has appeared at length in the newspapers, as well as the addresses of the Presidents of the various Sections, that of greatest interest to our readers being the address of Dr. Schüster, President of Section A. This address, though comparatively short, was very suggestive. After reviewing the work recently done, he suggested various important subjects for investigation which, it is to be hoped, will stimulate the many students in this branch of research. He made the following interesting remarks on the scientific amateur:—

"By amateur I do not necessarily mean a man who has other occupations and only takes up science in his leisure hours, but rather one who has had no academical training, at any rate in that branch of knowledge which he finally selects for study. We may, perhaps, best define an amateur as one who learns his science as he wants it and when he wants it. I should call Faraday an amateur. He would have been impossible in another country; perhaps he would be impossible in the days of the Science and Art Department. Other names will occur to you, the most typical and eminent being that of Joule. We are in danger of losing one great and necessary factor in the origination of scientific ideas. If I am right, there is a distinct advantage in having one section of scientific men beginning their work untrammelled by preconceived notions, which a systematic training in science is bound to instil. The chief difficulties in which the Universities are placed at present are due to the fact that they have given way too much to outside advice; but I cannot help expressing a strong conviction that their highly specialized entrance examinations are a curse to all sound school education, and will prove a still more fatal curse to what concerns us most nearly—the progress of scientific knowledge. If school examinations could be more general, if scientific theories could only be taught at an age when a man is able to form an independent judgment, there might be some hope of retaining that originality of ideas which has been a distinctive feature of this country and enabled our amateurs to hold a prominent position in the history of science. At present a knowledge of scientific theories seems to me to kill all knowledge of scientific facts. It is by no means true that a complete knowledge of everything that has a bearing on a particular subject is always necessary to success in an original investigation. In many cases such knowledge is essential, in others it is a hindrance. Different types of men incline to different types of research, and it is well to preserve the dual struggle. The amateur supplies the steam, and the Universities supply the cold water; the former, boiling over often with ill-considered and fanciful ideas, does not like the icy douche, and the professional scientist does not like the latent heat of the condensing steam, but nevertheless the hotter the steam and the colder the water the better works the machine. Sometimes it happens that boiler and cooler are both contained in the same brain, and each country can boast of a few such in a century; but most of us have to remain satisfied with forming only an incomplete part of the engine of research. But while it is necessary to recognize the great work done by the unprofessional scientists, it seems not untimely to draw their attention to the damage done to themselves if they overstep their legitimate boundaries, and especially if they seek popular support for their theories, which have not received the approval of those who are competent to judge. An appeal from Alexander sober to Alexander drunk will not prove successful in the end. The gradual disappearance of the amateur may be a necessary consequence of our increased educational facilities, and we must inquire whether any marked advantages are offered to us in exchange. There is one direction in which it would seem at first sight, at any rate, that a proper course of study could do much to facilitate the progress of research. If the discoverer is born, and cannot be made, would it not be possible at any rate to train the judgment of our students so that they may form a sound opinion on the new theories and ideas which are presented to them? Is there much evidence that great discoveries have been seriously delayed by want of proper appreciation? Those who experience a temporary check, owing to the fact that public opinion is not ripe for their ideas, are often amply rewarded after the lapse of a few years. The disappointment which Joule may have felt during the time his views met with adverse criticisms from the official world of science was no doubt amply compensated by the pleasure with which he watched the subsequent progress of research in the new domain which his discoveries have opened out. The judgment of the scientific world seems to me to be tending too much towards leniency to apparently absurd theories, because there is a remote chance that they may contain some germ of real value. Worse than bad theory or logic is bad experimental work. Should we then not rigorously preserve any influence or incentive which encourages the beginner to avoid carelessness and to consider neither time nor trouble to secure accuracy? The prospect of admission to the Royal Society has been most beneficial in this respect, and the honourable ambition to see his paper published in the 'Transactions' of that society has preserved many a student from the premature publication of unfinished work."

Speaking of the subjects for investigation, he said:—

"The statement of a problem occasionally helps to clear it up, and I may be allowed therefore to put before you some questions, the solution of which seems not beyond the reach of our powers:— (1) Is every large rotating mass a magnet? If it is, the sun must be a powerful magnet. The comets' tails, which eclipse-observations show stretching out from our sun in all directions, probably consist of electric discharges. The effect of a magnet on the discharge is known, and careful investigations of the streamers of the solar corona ought to give an answer to the question which I have put. (2) Is there sufficient matter in interplanetary space to make it a conductor of electricity? I believe the evidence to be in favour of that view. But the conductivity can only be small, for otherwise the earth would gradually set itself to revolve about its magnetic pole. Suppose the electric resistance of interplanetary space to be so great that no appreciable change in the earth's axis of rotation could have taken place within historical times, is it not possible that the currents induced in planetary space by the earth's revolution may, by their electromagnetic action, cause the secular variation of terrestrial magnetism? There seems to me to be here a definite question capable of a definite answer, and, as far as I can judge without a strict mathematical investigation, the answer is in the affirmative. (3) What is a sun-spot? It is, I believe, generally assumed that it is analogous to one of our cyclones. The general appearance of a sun-spot does not show any marked cyclonic motion, though what we see is really determined by the distribution of temperature, and not by the lines of flow. But a number of cyclones clustering together like the sun-spots in a group should move round each other in a definite way, and it seems to me that the close study of the relative positions of a group of spots should give decisive evidence for or against the cyclone theory. (4) If the spot is not due to cyclonic motion, is it not possible that electric discharges setting out from the sun, and accelerating artificially evaporation at the sun's surface, might cool those parts from which the discharge starts, and thus produce a sun-spot? The effects of electric discharges on matters of solar physics have already been discussed by Dr. Huggins. (5) May not the periodicity of sun-spots, and the connexion between two such dissimilar phenomena as spots on the sun and magnetic disturbances on the earth, be due to a periodically recurring increase in the electric conductivity of the parts of space surrounding the sun? Such an increase of conductivity might be produced by meteoric matter circulating round the sun. (6) What causes the anomalous law of rotation of the solar photosphere? It has long been known that groups of spots at the solar equator perform their revolution in a shorter time than those in a higher latitude; but spots are disturbances which may have their own proper motions. Duner has shown, however, from the displacement of the Fraunhofer lines, that the whole of the layer which produces these lines follows the same anomalous law, the angular velocity at a latitude of 75° being 30 per cent. less than near the equator. As all causes acting within the Sun might cause the angular velocity of the Sun to be smaller at the equator than at other latitudes, but could not make it greater, the only explanation open to us is an outside effect, either by an influx of meteoric matter, as suggested by Lord Kelvin, or in some other way. If we are to trust Dr. Welsing's result, that faculæ which have their seat below the photosphere revolve in all latitudes with the same velocity, which is that of the spot velocity in the equatorial region, we should have to find a cause for a retardation in higher latitudes rather than for an acceleration at the equator. The exceptional behaviour of the solar surface seems to me to deserve very careful attention from solar physicists. Its explanation will probably carry with it that of many other phenomena."

Speaking on a subject that has certainly exercised the minds of certain critics of the British Association, he said the following very good words in its favour:—

"In conclusion, I should like to return for an instant to the question whether it is possible by any means to render the progress of science more smooth and swift. If there is any truth in the idea that two types of mind are necessary, the one corresponding to the boiler and the other to the cooler of a steam-engine, it must also be true that some place must be found where the two may bring their influence to bear on each other. I venture to think that no better ground can be chosen than that supplied by our meetings. We hear it said that the British Association has fulfilled its object; we are told that it was originally founded to create a general interest in scientific problems in the towns in which it meets; and now that popular lectures and popular literature are supposed to perform that work more satisfactorily, we are politely asked to commit the happy despatch. There is no need to go back to the original intention of those who have founded this institution, which has at any rate adapted itself sufficiently well to the altered circumstances to maintain a beneficial influence in scientific research. The free discussion which takes place in our Sections, the interchange of ideas between men who during the rest of the year have occupied their minds, perhaps too much, with some special problem, the personal intercourse between those who are beginning their work with sanguine expectations and those who have lost the first freshness of their enthusiasm, should surely one and all insure a long prosperity to our meetings. If we cannot claim any longer to sow the seeds of scientific interest in the towns we visit, because the interest is established, we can at any rate assure those who so kindly offer us hospitality that they are helping powerfully in the promotion of the great object which we all have at heart."

Professor Sir G. Stokes, in moving a vote of thanks to the President for his address, said he was glad the President had brought before the notice of the Section these interesting problems which lay on the borderland of physics and astronomy. He would like to remind the President that in concluding by a statement of questions hitherto unanswered he was following the example of Newton, who at the end of his Optics enumerated several problems which were then unsolved.

Lord M'Laren seconded the motion, which was carried by acclamation.

The work of the Section did not treat of any astronomical subject interesting to our readers excepting a very important paper by Professor Michelson, of America, on the Application of Interference Methods to Spectroscopic Measurements. This paper we hope to be able to give at length at some future time. In section G Dr. Schlichter read a paper on a very ingenious application of celestial photography—a method of obtaining Longitudes by photographing the Moon and a star.

Taken as a whole, the Meeting was one of the most successful that has been held of recent years. As the 'Times' says—"The Edinburgh Meeting has been in many ways a marked contrast to that which was held last year at Cardiff. There all was dulness and depression; in the 'Gay Metropolis of the North' all has been brightness and exhilaration. There has only been one bad day; bright sunshine and a bracing air have prevailed throughout the week."

THE ASTRONOMICAL SOCIETY OF THE PACIFIC.

Minutes of the Meeting held at the Lick Observatory, June 11th, 1892.

President Schaeberle presided.

Eighteen new members were elected.

The Committee on Publication was authorized to print a limited number of suitable advertisments at the end of each number of the Publications, at the following rates :—

1 page, for one year	$40
½ ,, ,, ...	20
¼ ,, ,, ...	10
1 page, for 1 insertion	10
½ ,, ,, ...	6
¼ ,, ,, ...	5

Mr. Pierson, of the Committee on Observatory in San Francisco, reported that the Park Commissioners had kindly granted a site in Golden Gate Park for the proposed Observatory.

Special attention was called to the beautiful album of engravings of the buildings and instruments of the Nice Observatory, presented by Mr. Bischoffsheim, the founder of the Observatory, and to the very rare medal of the Great Comet of 1680, presented by Professor Holden.

The following programme was presented:—

"Lunar Eclipse of Jan. 1888," by Professor Weinek, of Prague.

"The Proper Motions of Stars with different Spectra," by W. H. S. Monck, of Dublin.

"The McCormick Observatory," by H. A. Sayre, of the University of Virginia.

"A Trial of Foucault's Pendulum Experiment," by T. Köhl, of Denmark.

"Notice regarding the Astronomical Exhibit at the World's Fair," by Geo. E. Hale, of Chicago.

Verbal Accounts, by Profs. Schaeberle and Campbell, of observations of the Spectrum and Magnitude of the New Star of 1892; of Photographic Observations of the Great Sun-spot of February 1892; and of observations of the Spectrum of Swift's Comet. Illustrated by photographs made at the Lick Observatory.

Recent enlarged photographs of Venus and of Saturn made with the 36-inch equatorial and an enlarging-lens by J. A. Brashear were exhibited to the members, as well as similar photographs, by Prof. Schaeberle, of the region near the New Star in Auriga; photographs and drawings, by Prof. Campbell, of the Spectrum of the New Star and of Swift's Comet; and some very successful photographs of stellar spectra taken in the 2nd and 4th orders of a Rowland grating of 14,000 lines. F. R. Ziel, *Secretary.*

Nova Aurigæ *.

Nova Aurigæ will always be memorable as the first temporary star to the investigation of which chemical methods were applied. Their application, moreover, had a special fitness to the nature of the apparition, since its most noteworthy feature was the spectroscopic display of motion in the line of sight, for the measurement of which photography offers particular advantages. The spectrum of the new star was indeed of an extraordinary nature. It was two-fold. It included a long range of vivid lines extending very far into the ultra-violet, each with a dark line attached at its more refrangible side. The entire hydrogen-series from crimson C to the last of its rhythmically-disposed associates were thus doubled, as well as the calcium line K, the sodium D and others. Many more, at the same time, belonging to the crowd of feebler bright lines, so to speak, threw no shadows; while every well-marked dark line seemed connected with a bright one. All, however, that could be terrestrially identified proved to be displaced from their normal positions—the bright lines towards the red, their dark companions, to a still greater extent, towards the blue. Evidently, then, two oppositely moving bodies were concerned in producing a compound spectrum, the gaseous section of which testified to a recession from the earth of about 230 miles a second, while the juxtaposed absorption-rays crossed the otherwise continuous spectrum of a mass advancing with the exceptional, though not altogether unprecedented, swiftness of 320 miles per second. Thus, a telescopically single star was resolved, by the analysis of its light, into two physically unlike, though chemically similar, components, accomplishing a daily journey of separation extending over close upon fifty millions of miles! Possibly even a good deal more. Spectroscopic estimates of motion are always minimum values. They take into account only that portion of velocity directed along the visual ray; they necessarily neglect the portion directed across it. We are, however, in absolute ignorance as to how much of the speed of Nova Aurigæ was thus directed. All that can be said is that the chances are enormously against the whole of it having been measured on the Potsdam and Tulse Hill negatives.

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* A paper read before the Astronomical and Physical Society of Toronto, July 12th, 1892.

Nova Aurigæ was then really, though not optically, a double star; and there can be no question but that its luminous outburst sprang from its temporary duplicity. The outburst was very sudden. On December 8, 1891, Dr. Max Wolf, of Heidelberg, took a photograph of the sky around χ Aurigæ, showing stars to the ninth magnitude, on which the spot subsequently occupied by the Nova was blank. Yet it imprinted itself, forty-eight hours later, as a bright star of the fifth magnitude, on a plate exposed at Harvard College; and attained a maximum, similarly recorded, of about 4·5 magnitude, on December 20. But these photographic documents, as every one interested in the subject is aware, were only looked into after the visual discovery of the star by Mr. Anderson, of Edinburgh, on February 1, 1892. All the information collected about its spectrum, and the extraordinary revelations of movement afforded by it, were accordingly of a later date. This was unfortunate. One single determination of the kind in December would have been worth many in February and March. For it was then, presumably, at the time of the stars' abrupt leap upwards from obscurity, that they made their nearest approach to each other; and at that critical epoch the changes in their velocities must have been comparatively rapid. Fifty days later they had, as it were, settled to their stride, and were rushing apart at a rate the variation of which from uniformity fell nearly, it not quite, within the limits of probable error in measurement.

One point, however, seems perfectly clear. Their velocity was hyperbolic. That is to say, it was not all due to the influence of their mutual gravitation. The bodies animated by it were very far from being relatively at rest when they began to fall together. Each, on the contrary, must have possessed a swift motion of its own pursued along a line nearly opposite to or intersecting that of the other. These assertions can be easily justified.

We may fairly assume that the components of Nova Aurigæ passed periastron about the time of their greatest brightness, that is, on December 20. They had accordingly been in course of recession from the scene of their close approach for some forty-four days when they began to be spectroscopically observed; yet their relative speed can then scarcely have fallen short of six hundred miles a second. It kept, at any rate, well up to 550, on February 22—sixty-four days after periastron—when Dr. and Mrs. Huggins took a fine photograph of the spectrum so curiously significant on the point. Nor had it perceptibly slackened a week or more later, judging from the spectrographic determinations at Potsdam. But this high and lasting speed cannot have been parabolic—cannot, in other words, have been the speed due to the simple falling together, from an indefinite distance, of two gravitating bodies. A combined mass *ten thousand* times that of our sun would be inadequate to produce the observed relative velocity of the components of the Nova; the mass of ten thousand suns is not an approximate value, but merely a minimum estimate! An elementary calculation shows that no less enormous a power would be required to produce, at the end of sixty-four days' retreat from a grazing encounter, a parabolic velocity of 560 miles a second, diminishing, at the end of seven additional days, to 537 miles a second. Few, however, will be prepared to admit so extravagant a demand in the way of mass; and there seems no alternative but to conclude the orbits of the separating stars to have been hyperbolas. A very large proportion of their movement might, on this view, be original or inherent, and would hence persist irrespectively of the lapse of time; the changing element due to the accelerative force of gravity sinking, by comparison, into insignificance. Moreover, the ascending branches of the hyperbolas traversed may be assumed to have come near coincidence with our line of vision. Otherwise, the measured velocities, extraordinarily high as they were, must have been considerably exceeded by the actual rates; which appears unlikely.

The meeting of the constituents of Nova Aurigæ must then have been casual (in our limited apprehension) and unique. It can never, in all time, be repeated. *Nulla vestigia retrorsum* is the inexorable law of hyperbolic travelling. And this is only what we should expect in the case of a temporary star, the distinguishing "note" of which is one solitary outburst. How such outbursts are occasioned is not indeed in all respects clear; yet the circumstances attending them have been rendered perceptibly less enigmatical by the recent apparition. This was certainly due to the mutual action of two cosmical masses, brought for a short time into close contiguity. Not into actual collision; since their motion survived, evidently without material diminution, an upspringing of incandescence which, accordingly, it could not have been spent in producing. Hence the manner of influence exerted has still to be defined and ascertained. Dr. and Mrs. Huggins incline to the view that it was of a tidal nature. And the bodily strains due to the pull upon each other of two voluminous spheroids, swinging round with possibly a bare escape of contact, must certainly have been of extreme intensity, and would be most likely to cause eruptions of heated gases from their interior parts, by which inert photospheric surfaces might be vivified to brief brilliancy. This hypothesis, too, would account, better perhaps than any other, for some of the minor peculiarities noted in the spectrum of the new star. Thus, the frequently multiple character both of the bright and of the dark lines contained in it, testified, it was plausibly argued, to the presence of "reversals," such as commonly occur above sunspots. They may be described as the dividing-up of wide bright lines by narrow dark ones, or of wide dark lines by fine bright rays; and they would be an almost inevitable accompaniment of enormous outpourings of ignited gases, ensuing upon tidal deformations. These were then presumably concerned—perhaps fundamentally concerned—with the phenomena of the Nova; but other causes may also have contributed to their production. The vividness of the transient illumination suggests, for instance, the agency of electricity rather than mere elevation of temperature; and electric forces of great potency can hardly fail to have been generated by the close approach, at high velocities and violent mutual disturbance, of two more or less sunlike bodies.

Their solar nature was attested by the character of their spectrum. It was in the main that of the solar chromosphere, but displayed through the means of lines of absorption by the member of the pair advancing towards the earth, while the receding mass showed the same lines brilliant by emission. None of the distinctive elements of nebular or cometary light were detected; and an extreme degree of luminous excitement was certified by the strongly-marked presence of the entire ultra-violet hydrogen-series of rays, besides that of other groups still more refrangible, of unknown origin. The combined spectrum, in fact, rivalled that of Sirius in the extent and proportionate strength of its most refrangible section.

The question is one of much interest as to how far Nova Aurigæ might be regarded as a typical "new star." Was it an object so peculiar that reasonings about it could not legitimately be generalized, or were the discoveries it gave rise to capable of extension to other members of its class? There can be very little doubt as to the answer. Although the apparition had a *specific* character of its own, its *generic* character was that of a true Nova. Its visibility lasted, it is true, longer than that of any of its congeners in the present century. Nova Ophiuchi (1848), T. Coronæ (1866), and Nova Cygni (1876), had each an exceedingly short maximum—to be counted, indeed, rather by hours than by days—while Nova Aurigæ did not enter upon its definitive and precipitate decline for nearly three months after its first manifestation. But the Pilgrim Star of 1572, and Kepler's Star of 1604, continued lucid during a year and a half and two years, respectively, and the triple maximum of Anthelm's Nova of 1670 stretched over three years; so that the rule of speedy extinction admits of a fairly wide interpretation. Virtually the same range of bright lines seem, moreover, to have been observed in the spectra of the stars of 1866, 1876, and 1892. All three objects may, accordingly, be referred without hesitation to the same category; and all three must have attained brilliancy through the action of analogous causes. Nor can any valid distinction be drawn between these and other new stars, each of which must hence have represented the transitory conjunction of two bodies happening to pass very close by one another as they swiftly pursued their ways through space. But this state of things might not always disclose itself to observation. The velocities of the rushing masses might often escape even the detecting power of the most stringent light-analysis. If directed across our line of sight, they would be spectroscopically annulled; if much inclined to it, they would be so much reduced in amount as easily to evade notice. Further, there is no obvious reason why a gaseous should be strictly matched, as in Nova Aurigæ, by an absorption spectrum. But even if it were, the two might not be readily, if at all, distinguishable, since they would depend for their separate conspicuousness upon the oppositely displacing effects of rapid motion in the line of sight. It was this pushing apart of lines identical in origin that made the duplication of the spectrum, in the case of the recent star, so remarkable and unmistakable. Had radial movement been absent, the bright and dark series would have coalesced, and neither might have been distinctly perceptible. For in the integrated light, absorption would so nearly neutralize emission as to give the appearance, if not of a perfectly continuous spectrum, at least of one feebly characterized by those secondary bright lines which were uncompensated by dark ones. Thus, the spectrum of Nova Andromedæ (1885) was of this kind, and might conceivably have been so produced. The traces of bright lines noted in it might have represented, not the totality of emission, but only the excess of emission over absorption. They stood perhaps for the algebraic sum of many positive and negative quantities. This condition is periodically illustrated by the well-known short-period variable, β Lyræ, the only star yet known to exhibit a compound spectrum like that of Nova Aurigæ. It affords evidence, however, as might have been anticipated, not of hyperbolic, but of approximately circular motion. The bright lines, instead of remaining fixed beside the dark ones, shift to and fro, so as to be seen alternately on either side of them, conformably to the period of nearly thirteen days, in which β Lyræ varies in lustre. At intermediate epochs, moreover, when the orbital velocity of the star is tangentially directed, and its spectroscopic effects are consequently reduced to zero, the bright lines tend towards disappearance, overpowered by their dusky companions. In Nova Aurigæ, too, a similar effacement of mutually counterbalancing appearances should have marked (had observations then been practicable) the short interval during which its components wheeled round each other, preparatory to entering upon their endless career of separation. From a different point of view, indeed, this effacement would have been permanent. Had the orbit of the Nova been tilted up so as to lie nearly square to our line of vision, its spectrum would have borne a comparatively indeterminate character. The separate elements combined in it would have been inextricably confused; neither visual nor chemical methods could have afforded the means of reading its meaning aright.

68 Redcliffe Square, London, S.W. AGNES M. CLERKE.

Cometary Notes.

[Concluded from p. 312.]

CONCERNING the interest which attaches to the theory of the capture of comets by planets, as proved by the many contributions that have been made to the literature of the subject, it is not surprising that Prof. Newton, one of the earliest to work in this direction, should be tempted to renew his promising investigations which, commenced in 1878, have not been vigorously pursued till recently. The condition in which the original researches of Prof. Newton left the problem was to derive a formula to express the change of energy which a comet experiences when coming within the sphere of attraction of a planet. In this discussion the comet was regarded as moving first in a conic section about the Sun as focus, and subsequently, and while under the influence of the planet, on a branch of an hyperbola with the planet in the focus. Professor Newton now discusses, in considerable detail, an expression which connects the semi-axis major of the two orbits, before and after the approach to the disturbing planet. As the comet is generally supposed to approach with parabolic motion, the semi-axis major, in the first case, is infinite, and a convenient expression for the mean distance of the comet from the Sun is given in terms of the direction in which the comet approaches the disturbing object and the distance between the orbits at closest approach. This equation in its most convenient arrangement involves three independent variables, and all the possible cases of approach of the comet to the planet, so far as change of the axis major is concerned, may be represented by giving to them varying values. In this way Prof. Newton exhibits a series of diagrams, which show for various values of ω (the angle between the tangents to the orbits of Jupiter and the comet drawn through the point of closest approach) the form of the curve along which the total action of the planet upon the comet will be to reduce the velocity of the comet by a constant amount. From these diagrams, and the tables from which they are constructed, we learn that if a comet approach Jupiter in such a manner that an angle of 10° is included between the directions of the original motions, the orbit may be at once reduced from that of a parabola to a moderate ellipse, whose mean distance is 3·04 (Earth's distance = 1). But to accomplish this feat it is necessary that the orbits actually intersect, and that the comet should be the first to arrive at the point of intersection, Jupiter at that moment being distant ·0125 R. If the comet arrives *after* the planet at the point of intersection the result is very different, for if Jupiter be distant ·15174 R the resulting orbit would be hyperbolic, with a semi-transverse axis of 36·90 R.

The maximum effect of perturbation, however, occurs when the path of the comet cuts the orbit of Jupiter at an angle of 45°, and, when the comet having arrived at the point of intersection, the disturbing planet still lags behind by a distance equal to that of the planet from the Sun, multiplied by the planet's mass. In the case of the Earth, this point would be about 270 miles from the Earth's centre, and actual collision would prevent the exhibition of the greatest disturbing effect; but on Jupiter, with greater mass and greater distance, this point lies so far from its surface that the maximum diminution of major axis is possible. In this case the comet would be left entirely at rest, to fall to the Sun.

Intimately connected with these inquiries is the question how many comets are likely to experience this violent perturbation sufficient to change the form and character of the orbit. To solve this problem, Prof. Newton makes two assumptions which do not seem to be at all improbable. He supposes that on the surface of a sphere described about the Sun with arbitrary radius r, space is filled equally with comets, and that the directions of the comets in each unit of space are at random. Making these two assumptions, he inquires how many comets in 1000 millions would have their mean distances reduced to be comparable with that of Jupiter. The result is as follows:—

Comets with periodic times less than			$\frac{1}{2}$ Jupiter ..	126
,,	,,	,,	Jupiter	839
,,	,,	,,	$\frac{3}{2}$ Jupiter ..	1701
,,	,,	,,	2 Jupiter ..	2670

If we confine our attention to the 839 comets whose periods are less than that of Jupiter, it will be found that 257 would move in orbits with direct motion, and at an angle inclined to the ecliptic of less than 30°, while the number of those inclined to the ecliptic at an angle greater than 150°, or with retrograde motion, would be only 51, or more than five times as many comets might be expected with direct as with retrograde motion.

At first sight these numbers seem to tell against the probability of the introduction of comets of short period into the solar system through the action of Jupiter. But it has been already pointed out in these pages that Jupiter can gradually supply the means for their introduction by successive perturbations, and that every approximate encounter between the planet and the comet at its successive returns paves the way for its final capture, and tends to restrict the path to the limits of the solar system.

Brorsen's Comet.—The history of Brorsen's comet is as interesting as its disappearance is inexplicable. First seen in 1846, it was soon proved by Hind, Brunnow, and D'Arrest to be moving in an ellipse of the short period of 5½ years. Indeed, so nearly is this the period, that the returns to perihelion have always occurred in the spring and autumn alternately. The spring returns have always been favourable for observation, the comet being thrown into the circumpolar regions of Central Europe, but in the autumn visits the comet has remained near the equator and only visible for a short time before sunrise. In 1851, the comet was not seen; several circumstances contributed to this untoward result. In 1857, when the predicted date of perihelion passage was 87 days in error, the comet was fortunately found by Bruhns accidentally. The comet again passed without being detected in the autumn of 1862, but the careful calculations of Bruhns enabled that astronomer to find it himself in 1868. At the next return, in 1873, the comet was found at Marseilles by M. Stephan, assisted by a hastily prepared calculation by the writer. This was the only autumn return at which the comet has been observed, and it remained a faint and difficult object and its place was very inadequately determined; but in the spring of 1879 it was followed at many observatories up to May 23, when it was last observed at Leipzig. The comet has never been seen since.

The orbit exhibits some curious features. The inclination is 29°, much greater than that of any other short-period comet; and though this inclination might be expected to carry the comet far from the range of planetary perturbation, as a matter of fact, it can at its ascending and descending nodes approach both Venus and Jupiter respectively. Dr. E. Lamp, whose monograph on this comet is the reason for this note, points out that with the elements of 1846 the comet can approach Venus within 0·064 and Jupiter 0·045 (Earth's distance = 1). In 1842 this minimum distance was very nearly attained, with the result that a very different orbit was conferred, the inclination being reduced from 46° to 29°. We had been led to expect another close approach in 1937, when the comet might have been deflected into another path, which would have hidden it from us for ever. This catastrophe the comet has apparently anticipated by prematurely removing itself from our system.

The circumstances of its return in 1890 were as favourable for observation as at any previous occasion. It was sought by powerful instruments and experienced observers ineffectually, and there seem but two possible causes to explain its disappearance—either the comet must have dissipated itself or the calculations of its expected path must be in error. With the view of removing the possibility of the second of these causes, Dr. E. Lamp has given in detail his computations, which apparently remove one of the obvious explanations.

In considering the possibility of dissipation, it must not be forgotten that the comet might have come into collision with an asteroid, either known or undiscovered. Such a cause has been suggested with some plausibility for the dichotomized appearance of Biela's comet, and it is not a little curious that Dr. Lamp finds that Brorsen's orbit can approach within the following limits the orbits of

Hebe	0·073
Hesperia	0·043
Artemis	0·107

These distances do not seem very close, but small errors in either orbit can much diminish them, so that collision does not seem impossible. Dr. Lamp, however, seems to prefer an explanation based upon the varying brilliancy of the comet at its successive returns. The theoretical brilliancy of the comet was greater in 1846 than any subsequent return; but Dr. Lamp contends that the comet was really brighter in 1857, when Dr. Schmidt at Olmutz reported that he saw it with his naked eye. At other perihelion passages there appear to be slight discrepancies between the computed brilliancy and the descriptions of observers, and on these slight fluctuations our author cherishes the hope that the comet may recover its brilliancy and be again observed at some future return. Perhaps with less reason, we take a more pessimistic view, and believe that, as the past history of Biela and De Vico teaches, the tendency of comets is to disappear.

W. E. Plummer.

Selenographical Notes.

Cassini.—Except under a comparatively low Sun, this very interesting and abnormal type of ring-plain is inconspicuous, while at lunar noon its attenuated walls are all but invisible, the position of the formation being only indicated by the brighter segments of the rims of A and *b*, the large depressions on the floor. Although an unsatisfactory telescopic object under most conditions of illumination, it is a very remarkable one when its eastern wall is on the morning terminator and for about twenty-four hours afterwards. The former phase affords a view of sunrise on the Caucasus and the Palus Nebularum which is hardly exceeded in beauty by any other lunar landscape seen under similar circumstances. Cassini at this time is filled with shadow or nearly so, the only objects visible within the ring being a portion of the large crater A and the somewhat smaller crater *b*. The last appears to be complete, without any signs of gap or interruption; while only the northern segment of A and a short piece of the southern rim and of the ridge which abuts on the south-western side of it are in sunlight. The narrow walls exhibit many breaks, especially on the south-west, and the broad western *glacis* appears to be pitted with irregularly-shaped hollows and covered with ridges. Just outside the wall, on the north, is a large depression filled with shadow, of which traces are shown in almost all the drawings I have made under oblique morning light, though it is not referred to by Neison. It lies on the west of the little crater which he shows on the outer slope of the north-east wall in Map III., and resembles very closely a formation seen under similar conditions of illumination south of Aristillus. I have a drawing of Cassini made by Mr. G. T. Davis of Reading with a 3·75 achromatic, when the long. of the morning terminator was about 3° east, which shows this object as a large oblong space enclosed on all sides, except the north, by steep walls; and another sketch by Mr. G. P. Hallowes, F.R.A.S., of Didsbury, made with an 8½-inch speculum under evening illumination, in which no trace of it appears, but on a portion of the site a small crater is shown, agreeing apparently in position with one drawn by Schmidt on the western side of a large circular enclosure, which possibly represents the depression in question. In a very good drawing made under morning illumination by M. Gaudibert on June 9, 1886, 9^h, and reproduced in Vol. V. of the 'Journal of the Liverpool Astronomical Society,' this object (lettered 12) resembles a large shallow hollow with a concave bottom. Two little mounds are shown on its north-western rim which, I think, represent Mr. Hallowes' little crater. A smaller triangular depression on the north of 12, and the crater drawn by Neison on the east of it, are both shown as filled with shadow.

The larger of the two ring-plains on the floor, A, is said to have a central peak, but it is doubtful whether what appears to be such is not really an irregularity on the inner slope of its north wall, or, as Webb, who observed it as long ago as September 1, 1836, suggests, "the edge of an interior and deeper crater." Schmidt draws a curious bending-in of this north wall. A suitable opportunity should be sought to determine this matter.

In one of Schröter's drawings of Cassini which is of a less conventional type than usual, he shows a long elevation extending from the south-west wall of A, and tapering to point near the inner foot of the south-west border of the surrounding ring-plain.

Mädler exhibits no trace of this rather obvious feature, but draws a good-sized crater adjoining A on its south-west side. That there is a mass of rocks, or what seems to be such, in the position indicated by Schröter, is shown by many drawings made under various angles of illumination during the last thirty years. There is a crater-like object also on the eastern flank of the rocky elevation, but whether it is a true crater or merely a circular indentation on the eastern face of the rocks it is difficult to say. I have drawn it as a crater on several occasions, both under a rising and setting Sun, and at other times have seen it as a semicircular hollow, open towards the east. Gaudibert is, I believe, the only observer, except Mr. Hallowes and myself, who has seen detail on the floor between A and *b*; in the sketch referred to above he shows three little hills in this position. Mr. Hallowes draws one, close to the eastern wall of A, and at 9^h on April 4 this year I saw another about midway between the two ring-plains, standing up above the edge of the spire of shadow east by A.

Schröter, Sel. Top. Frag. t. lxxiii. fig. 72, shows a circular depression outside the east wall, close to *b*. This object appears under various forms in many drawings in my possession; but, however much it may resemble a crater at certain times, it is really only a large "pocket" formed between the main wall and a somewhat less lofty ridge on the outer slope. On the south-west there are several of these curious structural features, which are very prominent when the long. of the morning terminator ranges from 0° to 1° east. There is also a small crater on the outer slope of this border, which Neison and Gaudibert record. I have only seen it on one occasion. The great extension of the *glacis*, or outer slope of the border, towards the west and its peculiar square-shaped termination are noticeable features. The slope evidently does not gradually merge into the surrounding plain, but is bounded by a cliff of some altitude, as is shown by the well-defined strip of black shadow which marks its position under evening illumination. There is a large circular depression, about equal to *d* in area, near the square-shaped end of the outer slope, which is always prominent under a low Sun.

On September 7, 1879, 11^h 30^m, Neison observed, with a power of 270 on a 9½-inch reflector, a conspicuous valley-like cleft extending in a bold curve from a break in the south wall of Cassini to a ridge projecting from the north wall of Theætetus. Schmidt shows one or two ridges in this position, but in all other drawings I have seen the site is devoid of detail.

Stretching southwards from the large crater Cassini *c* towards Theætetus is a faint marking, visible under a moderately high Sun, which represents the eastern foot of a gentle slope extending westwards towards the Caucasus, and still further west of this lies the cleft θ, which Neison shows as a straight continuous line running up to the eastern flank of Theætetus. I have had several satisfactory views of this object, notably at 9^h on April 4 this year, with a power of 350 on my 8½-inch Calver reflector. It is not continuous, but exhibits at least two short breaks, as shown by Schmidt.

Beaumont House, Shakespeare Road,
Bedford, 1892, August 19.

Thos. Gwyn Elger.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Herschel and Schröter.

Gentlemen,—

In the July number of the 'Observatory' you gave an account of M. Trouvelot's results of his observations on Venus. No one can read them without being struck by the fact to how great an extent they confirm the observations made by Schröter a century ago, the accuracy of which was so strenuously contested by Herschel in the 'Philosophical Transactions' for 1793, and re-asserted by Schröter in 1795. My present purpose, however, in referring to this controversy is to point out the danger of trusting to *translations* in matters of this kind, and the importance of referring in disputed points to the originals. Amongst the observations of Schröter to which Herschel alluded, in a tone which he must have afterwards regretted, were what he calls "flat spherical forms conspicuous on Saturn." What Schröter really wrote was "abgeplattete Kugelgestalt des Jupiter und Saturn," meaning flattened spherical shape of the planets themselves, not of markings on them, from which, had such been seen on Saturn, it would have been easy to deduce the exact time of its rotation.

Although Schröter does not allude to it, perhaps I may be permitted to point out another evident inaccuracy in the translation of his paper which appears in the 'Philosophical Transactions' for 1792; in the very same page (336), indeed in the very same sentence, as that represented in the English as speaking of "flat spherical forms conspicuous on Jupiter and Saturn." It runs thus:—"The circumstance also......gives room to infer that the globe of Venus, with reference to its diameter, and other circumstances of physical arrangement in its construction, performs its rotation round its axis in a much longer space of time than those planets or the nearly similar ones of our Earth and Mars; and this is actually confirmed by my observations on the diurnal period of Venus." Now certainly this language by itself without other sources of information would lead us to suppose that Schröter considered that Venus had a much longer period of rotation not only than "those planets" (*i. e.* Jupiter and Saturn), but than the nearly similar periods of the Earth and Mars. Yet we know that the period he deduced from his observations was $23^h\ 21^m$, somewhat less than that of either the Earth or Mars; so that we must again blame the translator and conclude that, in the above sentence, instead of "or the nearly similar ones of our Earth and Mars," the original meant "nearly similar to those of the Earth and Mars," *i. e.* similar as contrasted with the periods of rotation of Jupiter and Saturn.

The moral appears to be that in scientific matters, if it is thought necessary to give a translation of a paper, this should always be accompanied by the original, at any rate in passages where there is a possibility of misunderstanding the exact meaning.

Yours faithfully,

Blackheath, 1892, Aug. 3.

W. T. LYNN.

Tables of Uranus.

GENTLEMEN,—

Has it been ascertained from the few observations obtained of the occultation of Uranus on July 3 how nearly the present tables that are employed represented the phenomenon? Because, when Uranus was occulted in 1871, several observers (myself amongst them) on looking out for the planet were disappointed, as it had disappeared several minutes sooner than was expected. It was stated at the Meeting of the R. A. S. in March of that year that the tables of Uranus gave an error of 15″ in its places, and as the Moon moves 2″·5 in a minute, that would produce an error of 4 or 5 minutes in the time of the occultation. The tables used for the 'Nautical Almanack' at that time would be Bouvard's, published as far back as 1821, now superseded by the more accurate ones of Leverrier. Little notice seems to have been paid to these occultations of the planet previous to 1871. There was one in 1839, and also in 1824. Possibly the tables of Bouvard might have come into use for that one. Moore's Almanack for the year remarks on the star's great southern declination at the time (August 6) and the nearness of the Moon to the full, so that it would be "difficult to see this very distant star without the aid of a powerful telescope." Capt. J. Ross, in the high latitude of Stranraer, seems to have been provided, using a 25-feet reflector, the aperture of the mirror of which is not stated. He says the light of the planet began to "diminish before it touched the disk; and it appeared to have entered about one third of its diameter on the dark part of the Moon, at the same time separated by a fine line of light, before it disappeared; on the contrary, at its emersion it appeared one fourth of its diameter distant from the Moon's western limb." "The evening at my observatory was peculiarly favourable, and the telescope bore a power of 500 extremely well. Ther. 61°·30; bar. 29″·20." The existence of so large a telescope in so remote a part of the country 70 years ago is interesting, but one would like to hear whether other observations made by it are recorded. Is there an observation of an occultation of the planet previous to August 1824?

Faithfully yours,

Melplash Vicarage, Bridport,
1892, Aug. 10.

S. J. JOHNSON.

The naked-eye Visibility of Venus in Sunshine.

GENTLEMEN,—

Having seen Venus in daytime without optical assistance during several years past, I determined last October to note in what parts of her orbit she was so visible. The following record may therefore be interesting in connexion with M. Trouvelot's important observations published in the 'Proceedings' of the Astronomical Society of France. In consequence of the planet's north declination during the greater part of the period, the observations probably represent the visibility in our clear skies at any time. After superior conjunction in September last, Venus was first seen on December 30 as a difficult object, about an hour before sunset. She continued to be so until the beginning of February. The conjunction with Jupiter on Feb. 6 was unfortunately obscured by clouds. After this date she was an easy object, and during April and May was readily seen in full sunshine by many persons with only roughly approximate knowledge of her position. I found no trouble in seeing the planet up to July 1, generally observing about midday, but for two days previously it was necessary to shut out direct sunlight by means of adjoining buildings. July 2 and following days were hazy or clouded, or it is probable she would have been visible a few days longer. Since on July 1, when less than 14° from the Sun, Venus was more readily seen than on Dec. 30, although her distance from the Sun was then upwards of 27°, it is remarkable that M. Trouvelot should have found her visible nearer to the Sun at superior than at inferior conjunction. I shall continue my watch while she is west of the Sun. Venus has also been observed with an 8½-in. reflector, but no blunting of the cusps has been detected, although some of the dark markings have been visible.

Yours faithfully,

Paddington, N. S. W., 1892, July 11. WALTER F. GALE.

A New Variable Star in Norma.

GENTLEMEN,—

Mr. A. R. Roberts, of Lovedale, Alice, South Africa, informs me that he has discovered a new variable star of short period in the southern constellation Norma. The star is Lacaille 6743, and its position for 1880 is R.A. $16^h\ 8^m\ 56^s$, Dec. $-57°\ 36'\ 4''$. It is 6·6 magnitude in the 'Uranometria Argentina.' Mr. Roberts finds a variation from 6·5 to 7·2, with a period of 10 days, the interval from maximum to minimum being one day longer than that from minimum to maximum,—that is, the increase of light is quicker than the decrease, which is usually the case in these short-period variables. The comparison stars used were Lacaille 6827 (6·6 mag.) and Stone 8888 (7·2 mag.). The star lies close to Professor Pickering's "zone of short-period variables." As this is the first variable star which has been discovered in this constellation its designation will be R Normæ. Yours faithfully,

Ballysodare, Ireland, 1892, Aug. 9. J. E. GORE.

An excellent Lunar Photograph.

GENTLEMEN,—

It may interest some of your readers to hear of the excellence of one of the finest Lick Observatory lunar photographs, and which was taken at a comparatively recent date. The picture to which I refer was taken in the focus of the great telescope on the night of October 12, 1891, when the Moon's declination was —22°. The Moon's age was 10 days 10½ hours, the terminator being in approximately 35° east longitude. The Astronomical Society of the Pacific has recently been presented with an enlarged positive on glass made from the original negative. In this positive, which was originally intended for the Columbian Exposition, the Moon's diameter is about 17 inches. Although my examination of the positive has only been of a desultory character, I have been struck with its superiority over certain very fine silver prints I possess from photographs made at Mt. Hamilton in August 1888. For instance, in one of these photographs (in which the west wall of Copernicus is on the morning terminator) I can find with a magnifying-glass no satisfactory trace of the crater Piton *a*, and it is only with difficulty that I identify the crater west of it. The picture of October 12 shows these craters perfectly, in spite of the fact that they are so much further from the terminator, and also shows numerous other more difficult craters.

The most difficult crater I have picked up in the photograph is the second and larger of the two Schmidt maps S.W. of Piazzi Smyth. I estimated its diameter at two-thirds that of Piton *a*, in comparison with which it presents a very insignificant appearance, though perfectly shown in its true nature and easily visible without any other magnification than the enlargement gives. An interesting point in connexion with this crater is that in the photograph it is on the S.E. border of a bright streak extending from Piazzi Smyth towards Aristillus. In Schmidt's map it is on the border of a ridge which apparently is identical in position with the bright streak.

Neison's "minute craterlet" Gambart *g* is a conspicuous object, while three smaller craters, which divide the distance between Gambart C and the east wall of Schröter approximately into fourths, are plainly shown. Although the easternmost of these is the smallest, yet it is the one that Neison has selected to show. Other craters which the photograph shows very well are the double craters Plato *i* and *k*, also the conspicuous crater-pit on the floor of Clavius and near the northern wall just west of Neison's γ. The whole formation of Clavius is very well shown, in spite of the fact that it is carried far towards the Moon's south pole by libration. Raised walls are shown around all but the sixth of the great curved chain of ring-plains which stretches across Clavius.

In the plain between Copernicus and Eratosthenes a considerable number of depressions, in comparison with which

Stadius B is very conspicuous, are visible at the first glance, although they are more than 24 hours from the terminator. As for the photographic representation of Copernicus itself, I am more or less familiar with the principal published drawings of Copernicus, and do not think that any of them present such a combination of accuracy and detail as this simple but faithful reproduction of what could have been seen with a 3-inch refractor at the time the photograph was taken. If any drawing rivals this photograph, it is Secchi's. But the finest portion of the whole photograph is the mountainous region east of Copernicus. Only by personal inspection could one properly appreciate it. I do not consider it worth while to describe the positions of the small craters shown in this region, as I have already said enough to prove the capabilities of the photograph in that direction, but will refer to one on account of its being misplaced in Schmidt's map. S.E. of Mayer C a crater is shown at about the same distance from C as *d*. A line drawn from this crater to the centre of C would make an angle of 60° with the line joining C and *d*, whereas on Schmidt's map the angle is 90°.

The objects to which I have referred are all objects that can be readily seen, and are distinctly shown in their true nature. No doubt, by going over the photograph with a magnifying-glass much more difficult objects might be found, some of them probably too difficult to be seen with a 3-inch refractor except under perfect atmospherical conditions, the Moon's illumination, of course, being the same. But this brings us to another class of objects which the photograph cannot be praised for showing. At the first glance Thebit A appears to contain a prominent central peak, but on examining Schmidt's great map no such object is shown, yet a central peak is shown in Thebit *l*. It is utterly absurd to suppose Schmidt would have passed over such a prominent object as the peak in Thebit A must be, if it really exists as shown in the photograph, to note so small a one as the peak in Thebit *l*. It is evident that the dark marking in Thebit A, which appears to be the shadow of a central peak, must really be some surface-marking which the photograph has exaggerated. It is false to suppose that a photograph could show an object which casts a shadow better than it could be seen with the telescope, for the contrast between the black shadow and the illuminated surface of the Moon is not so strong in a photograph as in the telescope. On examining the photograph closely, a number of rills (an account of which Prof. Weinek has given in No. 22 of the 'Publications' of the Astronomical Society of the Pacific) are apparently seen in Longomontanus. Yet I have not been able to see these with the telescope and do not believe they have any real existence; that is, as rills. All which is sufficient to prove that we must be cautious in our interpretation of the markings shown in lunar photographs.

Yours truly,
ROGER SPRAGUE.

Berkeley, California, 1892, Aug. 8.

Fine Meteor.

Gentlemen,—

On Aug. 20, at 11^{h} 57^{m}, I observed a fine meteor, about as bright as Venus, flash out in Cassiopeia and traverse a path of 13° from 27° +63° to 27° +76°. Its motion was not very swift, and the path was evidently much foreshortened near a radiant at 27° +43°. The meteor left a streak for about half a minute projected almost upon the stars ε, 48, and 50 Cassiopeiæ. While watching the gradual decadence of the streak I saw a small meteor which had a short path of 5° across γ Cassiopeiæ and was evidently directed from the same radiant as the preceding one.

This shower in Andromeda is fairly conspicuous in August, supplying swift, streak-leaving meteors very similar to the Perseids. The radiant maintains its activity for a considerable time, and I have determined its position as follows:—

No.	Date.	Radiant.		
D. 305	1878 July 27–Aug. 1	23°	+41°	7 meteors.
D. 321	1886 August 4–6	26	+42	6 „
D. 401	1877 August 12–16	22	+46	7 „
D. 411	1884 August 19–21	25	+42	5 „
D. 441	1879 August 21–25	24	+42	9 „

Yours faithfully,
W. F. Denning.

OBSERVATORIES.

These notes on the work of some European observatories are extracted from the ‘Vierteljahrsschrift der Astronomischen Gesellschaft.’

Bamberg. *Ernst Hartwig.*—The large heliometer of 184 mm. aperture was brought into regular use. It had been used in 1889 for parallax observations of Victoria and Sappho. With it were made during 1891 twenty-one measures of position of the Lunar Crater Mosting A; measures of the equatorial and polar diameters of the Sun; measures of the lunar diameter in twilight and during the eclipses of May 23, Nov. 15, and 1892, May; measures of diameter of Mercury during the transit of 1891, May 9; observations of comets Zona, Wolf, Denning, Swift and Winnecke, &c. &c.

With other refractors observations of variable stars and a few occultations were made, besides observations for Prof. Kustner (A. N. 126) in connexion with the change in altitude of the pole.

Basel. *Dr. Riggenbach.*—Devoted to students. A balance and pendulum seismometer showed disturbances—

1891, Jan. 20^{d} 4^{h} 17^{m} $57^{s}.0$ Bern time.
„ June 7 1 46 55.1 „ „
„ Aug. 21 8 45 51.3 „ „

BERLIN. *Foerster.*—With the transit 1930 observations in R.A. and 1760 in N.P.D. were made. With the 9-inch equatorial a number of measures of double stars were made by Herren Knorre and See. The observations of the partial solar eclipse of 1891, June 6, and of the total lunar eclipse of 1891, Nov. 15, are published in 'Astronomische Nachrichten.'

BONN. *Küstner.*—Dr. Küstner, who was transit-observer at Berlin, succeeded Prof. Deichmüller on 1891, Oct. 1. Observation of zone-stars +40 to 50 with the transit was completed by the end of the year. Already the press copy for 15,000 stars of the great catalogue is complete, and the work is being pushed forward.

BRESLAU. *J. G. Galle.*—This observatory celebrated its 100th anniversary in 1891. Time-service and meteorological observations.

BUDAPEST. *Konkoly.*—Very little astronomical work was done here in 1891.

DRESDEN. *Dr. B. von Engelhardt.*—Micrometric measures of Bradley stars with a 12-inch Grubb equatorial. Observations of nebulæ, Comets 1890 II. (Brooks), 1891 I., 1891 V.; small planet Diana. Transit of Mercury on May 9 and total lunar eclipses of May 23 and November 16.

DÜSSELDORF. *Rob. Luther.*—Observations of minor planets and appertaining computations. 7-inch equatorial.

GENEVA. *R. Gautier.*—Col. Emile Gautier died on Feb. 24, and was succeeded in the directorship by his son. Great preparations were made to observe the transit of Mercury, the two lunar and one solar eclipse, but it unfortunately either rained or was clouded during each phenomenon. Time-determinations were made as usual with the small transit. With the 10-inch equatorial a number of observations of Comets 1890 II. and 1891 II. were made. Comet Winnecke was searched for in vain. Many evenings were devoted to the study of Saturn and Jupiter. The electric installation has been completed throughout the observatory. One marine and 398 pocket-chronometers were tested. Meteorological observations.

GOTTINGEN. *W. Schur.*—Triangulation of Præsepe with the 6-inch heliometer practically completed; a number of measures of the Sun's equatorial and polar diameters; measures for parallax of β Orion (Ast. Nach. 127) and also of ψ^5 Aurigæ and 70 Ophiuchi; special measures of the Pleiades for Dr. de Ball and in Perseus for Dr. H. Struve; determinations of position of Comets Wolfe, Encke, and Barnard-Denning. Measures of Jupiter, Saturn, and Uranus, and a number of miscellaneous objects. Regular meridian observations. Observations of Lunar surface.

GOTHA. *Paul Harzer.*—Exclusively for students.

HAMBURG. *G. Rumker.*—An interesting list of what may be termed working nights is given, viz.:—January 10, February 12, March 15, April 14, May 22, June 15, July 15, August 19, September 22, October 23, November 15, and December 16.

A large number of the fainter comparison stars were observed with the meridian circle. Meteorological observations. Rating of chronometers for the German navy.

HEIDELBERG. *Max Wolf.*—Possesses a 162-mm. equatorial. Photometric researches; photography of nebulæ and double stars; searching for small planets by photography (small planet no. 323 was found in this way); observations of meteors and comets.

In the course of the year 137 plates were exposed. The number of photographic nights in 1891 was January 10, Feb. 13, March 3, April 3, May 4, June 4, July 6, August 7, September 13, October 6, November 3, December 10; while the number of observation nights was half as many again. Time-service.

HERÉNY. *Eugen v. Gothard.*—The greater part of the year was occupied in the installation of numerous instruments; but time was found for spectroscopic researches and for continuing the photographic work of nebulæ and stars. The photographs especially mentioned are of G. C. no. 512–521, 1179, 1227, 4403, 4447, 4532, and α and γ Cygni.

JENA. *Otto Knopf.*—A new equatorial, 20-cm. objective and 3 metres focal length, has been set up and is found to be satisfactory. The transit of Mercury, the total eclipse of June 6, and the lunar eclipses of May 23 and Nov. 15 were observed with it (Ast. Nach. 127, 128). With the smaller refractor Comets 1891 II. and III. were observed. Variable star-observations. Time-service. Meteorology.

KALOCSA. *J. Fényi.*—The Sun was completely examined, on 157 days and partially on 40 days, for prominences. In all 1387 prominences of at least 20″ were observed. On 256 days the Sun's surface was examined for spots and faculæ, and 310 drawings by projection made. Only on 3 days was the Sun free from spots. Meteorological observations.

KIEL. *A. Krueger.*—"Die Centralstelle für Astronomische Telegramme." The refractor, 216 mm., has been used very largely for spectroscopic and photometric work, to the exclusion of position observations of comets.

KÖNIGSBERG. *C. F. W. Peters.*—Transit work. Measures of Struve's wide doubles. Annual parallax of stars. Lunar observations. Observations of Comets Wolf and Barnard-Denning.

KREMSMÜNSTER. *C. Wagner.*—The weather in 1891 was even more unfavourable for observation than in 1890, only 59 nights being available. Positions of the following were obtained with a 6-inch equatorial:—Hebe and Comets Barnard-Denning, Wolf 1891 II., Encke 1891 III., Tempel-Swift 1891 V. There were 210 days on which spots were observed on the Sun and 17 days when the surface was free from spots, the maximum number of spots being in July. Time-service and meteorological observations.

LEIPZIG. *H. Bruns.*—Parallax measures of stars with large proper motion; measures of Jupiter and Saturn; transit of Mercury on May 9. Total lunar eclipse Nov. 15; transit work zone +10° to +15°. Time-service. Meteorological observations.

LUND. *Folke Engström.*—Some 24,334 zone-observations are in course of publication. Three vols. have already appeared.

MILAN. *Schiaparelli.*—The 18-inch used for measurement of double stars. Preparing for a catalogue of 1100 stars, zone −2° to +6°, observed 1860–1872. Time-service and magnetic observations.

MÜNCHEN. *H. Seeliger.*—A new transit-circle, 6-inch aperture and 6-feet focal length, was mounted in July, and naturally much time has been occupied in determining instrumental constants &c., regular work beginning in November. With the 10½-inch refractor Dr. Bauschinger has measured Saturn and observed Comets 1890 II. (Brooks), 1891 I. (Barnard-Denning), and 1891 II. (Wolf). Some time has been spent in investigating personal equation dependent on the magnitude of stars. Meteorological observations.

O GYALLA (UNGARN). *Konkoly.*—Great part of the Director's time occupied in reorganizing the meteorological service. Sun observations on 175 days, being free from spots on 13 days. Maximum number in September. Meteors. Determination of longitude. Thirteen drawings of Jupiter with the Merz refractor, 254 mm. aperture. A few spectroscopic observations and some photographic experiments.

POTSDAM. *Vogel.*—The work on the motion of stars in the line of sight has already been published in the 'Monthly Notices' and the Ast. Nach. The 11-inch refractor has been used to observe the larger planets as well as comets and nebulæ. Photometric measures of 144 fundamental stars for the photometric Durchmusterung. Dr. Lohse has taken 50 photographs of the Sun. Dr. Spörer has examined the Sun on 278 days, and since April has not found one day on which the Sun was free from spots. The photographic refractor has been used for odd work. A mass of work on star-clusters, parallax of 61 Cygni, and photometric-photograph experiments. Meteorological work.

PRAGUE (University). *Weinek.*—Drawings of Lunar surface. Determination of latitude by the Talcott-Horrebow method. In all 1061 pairs of stars on 91 nights. Results in Ast. Nach. 3055. Good observation of transit of Mercury on May 10. No occultation could be observed during the total lunar eclipse of November 15, but the times of the shadow on the different lunar formations were noted. Observations of Jupiter's satellites. Time-service. Meteorological and magnetic observations.

Prague. *Safarik.*—The worst observing weather since 1875; there were 157 rainy days and 37 cloudy. Altogether Prof. Safarik had only 96 observing days, and part of this time he was unwell. A break in the clouds permitted a few observations during the lunar eclipse of November 15. Measure of Titan's diameter gave 0″·35. Observations of 124 Variable Stars.

Strassburg. *E. Becker.*—The 18-inch refractor was employed in determining the positions of Comets Zona, Spitaler 1890 II., Barnard-Denning, Wolf, Encke, and Tempel-Swift, also of nebulæ and comparison stars. Regular observations from a working catalogue of stars and of the Sun and larger planets were made with the transit-circle. The total solar eclipse of June 6 was observed, and a number of observations for determining the variation of latitude were made, beginning in 1891, May, and ending 1892, March. A large number of pairs of stars were observed each night, and the results show a steady increase in the altitude of the pole from May to September, after which it decreased again to the end of the series.

Upsala. *Dunér.*—Variable stars with 6-inch refractor. Time-service.

Zurich. *Rudolf Wolf.*—Sun examined on 320 days, and found free from spots on 18 of these. Observations for determining variation of latitude. Time-service.

PUBLICATIONS.

Double Stars *.—Prof. Asaph Hall's double-star observations made at the Washington Observatory with the 26-inch equatorial during the period 1880–1891 have all been collected and printed together as Appendix I. of the Washington Observations, 1888. It is scarcely necessary to say much concerning the excellence of the measures themselves; but we cannot allow the opportunity to pass of congratulating Prof. Hall on the production of a double-star catalogue containing measures of those interesting couples which have been so much neglected. The stars observed by every double-star observer have received just the amount of attention required and no more. It was quite a treat to enter the measures as star after star we found was just the one wanted. We are also inclined to say with Prof. Hall, "The formulæ and corrections for Personal Equation of observations seem to me of doubtful utility, and a better way is to compare the measurements of the same star with different observers." On page 25 the position-angle of Σ 158 should be 255°·3, and on page E 53 the distance of Σ 677 should be 1″·61. There are altogether 950 stars observed.

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* Washington Observations, Appendix I. Observations of Double Stars made at the United States Naval Observatory.—Part Second, 1880–1891. By Asaph Hall.

NOTES.

We print the following interesting letters from the 'Times':—

"A Much-neglected Astronomer.

"*To the Editor of the 'Times.'*

"Sir,—As the proposal to erect a memorial in the Westminster Chapter-house to a distinguished American has been received with favour, may I venture to call attention to a notable but much-neglected Englishman? In looking over the parish register of burials of Burstow not many days ago I discovered the following:—

1719.

"'Buried Jan. ye 12th The Revd. Mr. John Flamsteed, Rector of this Parish.'

"John Flamsteed, the first Astronomer Royal (which position he occupied for 43 years), made the earliest reliable catalogue of the fixed stars, described by his biographer, Francis Baily, more than a century afterwards, as 'one of the proudest productions of the Royal Observatory at Greenwich.' A most careful scientific observer, he supplied his distinguished contemporary, Sir Isaac Newton, with data for the illustration and verification of the Newtonian lunar theory. Between the years 1676 and 1689 Flamsteed made and recorded 20,000 observations of the stars, which he subsequently greatly increased, and he is allowed to be the founder of modern practical astronomy. Flamsteed made his own instruments at a cost of £2000, and paid his own assistants. His official salary at Greenwich was £100 a year, which he supplemented by teaching. Sir Isaac Newton has a splendid monument in Westminster Abbey; Flamsteed is without a memorial of any kind, either at Denby, near Derby, where he was born, or in the parish church of Burstow, Surrey, in the chancel of which his remains were deposited 173 years ago. It is proposed to remove the reproach by the erection of a suitable memorial window, with an appropriate tablet. Burstow is a large, thinly-populated rural parish, without town or village, though there are small scattered hamlets. Whether there are any astronomers living amongst us willing to co-operate in the project I know not, but some readers of the 'Times,' students of the starry realms, may be not unwilling to acknowledge indebtedness to Flamsteed in this way for his important astronomical researches. These researches were carried on with unparalleled ardour under great physical infirmities, until within a few days of his death. In reply to a recent letter on the subject, Mr. W. H. M. Christie, Astronomer Royal, writing from the Royal Observatory (also known as Flamsteed-house), Greenwich, August 10, says:—'It is strange that there should be no memorial of him (Flamsteed) in the church: I heartily concur in your proposal;' and the Astronomer Royal for Ireland, Sir Robert Ball, writes:—'You have rightly surmised that your proposal has my hearty sympathy, and if a subscription is got up my mite shall not be wanting.'

"John Flamsteed's portrait was painted by Gibson in 1712, but I have been unable to learn if it is still in existence.

"Yours faithfully,
"WILLIAM TEBB."

"Rede Hall, Burstow-by-Horley,
"Surrey, Aug. 19."

"SIR,—I may inform your correspondent, Mr. Tebb, that the original portrait of John Flamsteed, by T. Gibson, an engraving of which forms the frontispiece to his 'Historia Cœlestis Britannica' (edition of 1725), is in the gallery of the Royal Society at Burlington House. It was presented to the Society by Mr. John Belchier, F.R.S., who bequeathed to the Greenwich Observatory in 1785 a terra-cotta bust of Sir Isaac Newton, and from the terms of this bequest it would appear that the Flamsteed portrait had already been given by him to its present holders. The exact date of the gift, however, is not known.

"Your obedient servant,
"T. E. J."

"August 22."

COMET NOTES.—Swift's Comet may still be faintly seen with moderately sized instruments. The following ephemeris is by F. T. Archenhold, and is for Berlin Midnight:—

	R.A.			N. Decl.	
	h	m	s	°	′
Sept. 1....	0	41	24	52	38
5....		36	22	52	17
9....		31	8	51	49
13....		25	50	51	14
17....		20	33	50	32
Sept. 21 ..	0	15	24	49	45
25 ..		10	28	48	52
29 ..		5	52	47	53
Oct. 3 ..		1	36	46	50
7 ..	23	57	42	45	40

The brightness is steadily diminishing from 0·08 (compared with that at discovery) on Sept. 3 to 0·05 on Oct. 5.

The following ephemeris of Denning's Comet is by Herr Schorr:—

	R.A.			N. Decl.	
	h	m	s	°	′
Sept. 5....	6	26	21	22	51
9....		27	20	21	35
13....		27	57	20	16
Sept. 17 ..	6	28	10	18	55
21 ..		27	57	17	31

NOVA AURIGÆ.—Mr. Espin calls attention to the increase in brightness of Nova Aurigæ. On Aug. 21 it was examined at Wolsingham Observatory, and was found to be 9·2. Its spectrum was monochromatic with one intense line (500 ?).

ERRATUM.—No. 191, p. 318, line 24, *for* Wollatan *read* Wollaton.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 193. OCTOBER. 1892.

A Fifth Satellite to Jupiter.

The 'Standard' of September 13 had the following telegram from New York:—

"Professor Holden announces that the Lick telescope has revealed the existence of a fifth satellite to Jupiter, with a period of twelve hours thirty-six minutes; its distance from the centre of the planet is one hundred and twelve thousand four hundred miles, and its light compares with that of a star of the thirteenth magnitude."

Later telegrams inform us that Professor Barnard made the discovery and places the period at 17 hours 36 minutes.

From the telegram in the Ast. Nach. 3116, the announcement was dated Sept. 9, so that more than a fortnight has elapsed and no more information than this has been given on such an important discovery—not the slightest indication of its place in its orbit so that it could be observed elsewhere at its elongation without waste of time; we suppose that some explanation can be given why this necessary information has been withheld or at any rate has not appeared. In the meantime we must offer our hearty congratulations to the discoverer. Jupiter has been so much observed with large telescopes for so many years that it seems hardly possible that a satellite even as faint as a thirteenth magnitude star should have escaped; the honour due to Professor Barnard is therefore greater for his having found what so many have hitherto failed to see.

Mr. Freeman points out that the distance and the period are inconsistent; on this matter, as on the other, further information may throw some light.

Some Longitude Trips.

The attention of the Astronomer Royal has recently been directed by several independent external agencies to some fundamental longitudes. The late General Perrier in 1887 communicated with him on the question of a re-determination of the longitude Paris — Greenwich, for which there seemed to be two good reasons: first, that previous determinations were in several ways unsatisfactory, and, secondly, that the near completion of the geodetic survey of France suggested the present time as fitting for the more accurate connection of two fundamental points of the French and English surveys. Again, Dr. Otto Struve, on resigning the Directorship of the Pulkowa Observatory, declared it to be almost his last official wish that the longitude of Valentia, the western terminal point of the great arc of longitude extending eastwards to the Ural Mountains, might be re-determined for confirmation or correction of the adopted result. And, thirdly, negotiations for determinations of Transatlantic longitudes came almost simultaneously from Montreal and from Washington; and no doubt the adoption of Standard Time in America was partly responsible for both suggestions.

With the small staff of the Greenwich Observatory and the heavy regular work the Astronomer Royal has found considerable difficulty in complying with these requests; but every effort has been made to do so. The longitude of Paris was determined in 1888 by a pair of English and a pair of French observers independently. A curious discrepancy between the results obtained by the two pairs necessitated a re-determination in the present year, and not only doubled the work, but caused considerable trouble in discussion. Meanwhile in 1889 a triangle had been completed by the determination of the longitude Greenwich—Dunkerque by one English and one French observer. Next it was found that two or three birds might be killed with some possible saving of ammunition by choosing Montreal of the two Transatlantic Stations and deferring Washington for the present; for the Commercial Cable Company's cables, which had been generously offered by them for this purpose, came ashore at Waterville near Valentia, and the occupation of this station by an observer would not only facilitate the transmission of signals but would incidentally determine the longitude of Valentia: and the idea was extended to Canso, the Nova Scotian terminus of the cable, so that four stations were occupied throughout these operations,—Greenwich, Waterville, Canso, and Montreal, and all their relative longitudes have been determined. The operations were divided into two parts of about a month each—the first part occupying roughly 1892 April 15–May 25, during which there was no change of observers across the Atlantic; and the second part 1892 Aug. 15–September 20, for which an observer from each side crossed the Atlantic. The present year has thus been a very busy one for the Greenwich staff.

It has been my fortunate lot to take part in all these operations, and possibly the following rather rambling notes may have some interest for others.

The operations of 1888 and 1889 are rather too far back to dwell on at any length, though some of the incidents have not lost their vividness in my memory. For instance, there was the discovery at an early stage of the Paris operations that something had gone wrong with my transit instrument. We had had a week's bad weather to commence with, and this was the first fine day; and to find the collimation, which had been steady enough through the bad weather, now "unstable as water," was a little trying. It was some time before I found out that the object-glass was loose and devised a method of tightening it by poking cotton-wool with a quill toothpick between the outer lens and the ring holding it down. I do not make any claim to have forestalled Mr. Brashear in discovering the astronomical importance of the quill toothpick, for his use of it to clean the jaws of a slit is more harmonious and of more general application; but this temporary expedient for holding the object-glass was quite successful—which is more than can be said for several methods afterwards tried. I much doubt whether opticians have yet given sufficient attention to the proper mounting of object-glasses for different purposes. For instruments of precision both lenses must be held very tightly without distortion, which is of course a difficult but not an insoluble problem. For equatorials the holding tightly is not quite so important, but I remember wishing that the mounting could be made water-tight. It was on the 1886 eclipse expedition in Grenada, and after a night of tropical rain I found that in spite of all the protection available for the equatorial (including my mackintosh gaiters) water had got in between the lenses of the object-glass.

But Grenada is a long way from Paris—further even than Montsouris, which was the actual station for longitude operations and of which many Parisians have never heard. Away south past the Quartier Latin, and close to the fortifications, the French Service Géographique de l'Armée has established its little Observatory. The station is thus so far Paris that it is just inside the city walls, but is a "far cry" from the Paris of the English visitor. In the Parc de Montsouris, where on fête days there is high revelry, is a collection of buildings, including a meteorological observatory, an educational astronomical observatory, and the small meridian observatory where our observations were made, which is a transit building with three rooms, one for calculations, telegraphing, &c., and the other two equipped as transit rooms. The French observer was in one and the English observer in the other.

On fine nights when all went well the work was simple and pleasant. Each observer busily recorded transits and read levels. interrupting only to exchange signals with one or other of the observers at Greenwich, which took a few minutes on three occasions during the night. But there were times when day followed day without a break in the clouds, and hours of weary waiting for a chance of the sky clearing must be endured. One day when it had been raining steadily since noon, I went to the theatre, and enjoyed the first act immensely; but on looking out I found a cloudless sky and thus had an opportunity of stimulating the Paris Jehu to a mad gallop against time for the five miles to Montsouris: I have not preserved the exact figures of the record, but I remember it was under the hour, although the sky had had time to become quite cloudy. I did not again venture to the theatre, but we became so far callous in time as to light a fire in the outer room on rather hopeless nights; and towards the end of the series, the English and French observers might sometimes have been found playing *écarte*, with a soldier on the watch to report any change in the weather. We had perhaps chosen rather a bad time of year for the observations. We began on September 23 and did not finish till November 15. In the following year the longitude of Dunkerque was determined in June and July, with much greater celerity. But we have found in the present year that these months are not always fine, and even if one were at liberty to choose the time of year for such operations, it seems doubtful whether any sensible advantage could be gained, so long as the English climate is concerned.

Dunkerque is an important point in the French Survey for two reasons: it is practically the northernmost point of France, and the surrounding country is flat enough for the measurement of a baseline. The flatness is of course part of the same flatness which the Dutchman revels in; and nowhere does it impress one more than on the beach, where there are miles and miles of flat sand? Some hundred yards from the water's edge is a line of sandhills, countless bunkers in a continuous series such as might occur to a golfer in a bad nightmare. A man may walk on the flat sandy beach between the sea and the sandhills right into Belgium without any incident to speak of. In August there come troops of holiday makers, with children, who use the sand to the best advantage when the wind does not blow it about too much; for it is as windy at Dunkerque as it is at Sandwich.

In June the holiday makers had not yet arrived, and existence was of the tranquillest. I had some hours of very profitable amusement watching the *pêcheurs à la sonnette*, who make the fish ring the bell when he is hooked; and I marvelled that our Thames anglers have not yet in some such way managed to avoid the tedium of watching the float and secured an undivided attention for the bottled beer.

The observing station at Dunkerque (or rather at Rosendael-lès-Dunkerque) is on sandhills rather further inland. The soldiers who guard it and assist the observers were camped in tents all round it, and looked comfortable enough on fine days to make one rather envious. The officers of the Service Géographique can of course get the pick of the army to assist them, and it is considered a great privilege to be told off for duty at Montsouris, or Dunkerque or elsewhere in France, or even in the less attractive Algeria, where these officers have in recent years done so much good but arduous work.

With the actual observations for the longitude all went well. An interesting point arose in connection with the transit work, which ultimately resolved itself into a question of illumination of the wires. These were of sensible thickness and diffused daylight did not show them in the same place as the illumination by annular reflector at night, which suggested that the latter might be throwing the light on one side, as was found to be the case. While investigating this matter the general question of the proper method of using an annular reflector presented itself. In the ordinary course a beam of light diverges from a lens in one pivot to the reflector; but the brightest part of this cone of rays is generally in the centre and is lost through the hole in the reflector. The ring is only illuminated by the faint outer portions and thus a much brighter lamp is required for a given illumination of the field than would be necessary if the light could be used more economically. The chief reason for being sparing with the light is that a larger lamp is more likely to produce heating effects; and with the electric light this is not so much the case. But with the old oil lamp, especially when this is made small for the above reason, and the reflector is perhaps rather in need of regilding, there is a temptation to get a better illumination by throwing the beam obliquely into the pivot, so that the bright central portion falls on a small part only of the reflector, which often gives more light in the field than the whole ring illuminated symmetrically; and then the wires are shown slightly out of their proper place and a small error is introduced. When the wires are thin the error is very small, but in the determination of fundamental longitudes there are many similar points of minute detail which must be considered. H. H. Turner.

[To be continued.]

The Nova of 1892.

The astronomical event of the year is admittedly the arrival of a message from a distant region in the Milky Way, and delivered on a postcard at the Royal Observatory, Edinburgh, on the 1st of February. From thence the news spread rapidly over the northern hemisphere of the world; and every available instrument has been employed upon the scrutiny of the new spot of light near β Tauri, to decipher its meaning before it faded away.

The history of its discovery is too well known to bear repetition, and probably all the information touching its origin, collected at the best-equipped observatories, has been gathered together by Dr. Huggins, and given to us in the 'Fortnightly Review' for June last. Briefly the items of our knowledge are:—1. That whatever the nature of the catastrophe may have been, the development of heat was very rapid, and probably very great even in stellar computation. The star had made no impression on Max Wolf's plate exposed to that region on the 8th of December last, and two days later it left its mark upon the Harvard College plate of the 10th as a star of the 5th magnitude. The earlier plate showed stars to the 11th magnitude. So that the heat was great enough to raise the light-intensity of the unknown star fully 6 magnitudes within two days. The distance, however, is wanting to enable us to gauge the magnitude of the conflagration; but its position amongst the crowded stars of the Milky Way, where all the "Novas" have been found, makes it probable that it was beyond that of a measurable parallax; and Miss Clerke has given us the estimate, on this supposition, of one hundredfold of our own Sun's heat. 2. The light-intensity oscillated considerably before the steady decline set in, which ended in all but complete extinction about the beginning of April. 3. The spectrum was on the whole of the solar chromospheric type; its lines were very bright and wide, and many of them had dark companions on their more refrangible sides; and, further, the wide lines were striped with finer ones which were subject to considerable changes. 4. The origin of the spectrum had a double motion in opposite directions at the astonishing relative velocity of between 500 and 600 miles in the second, as indicated by the separation of the bright and dark lines. 5. This relative velocity remained practically unchanged during our six or seven weeks of observation. All these facts have to be considered in dealing with any answer to the question: What was the nature of the celestial catastrophe?

Authorities are generally agreed that the double spectrum, of bright and dark lines, must be attributed to separate sources, to two stars moving in opposite directions along our line of sight; and that the short-lived splendour of the pair is the denial of direct collision between the two masses. The enormous heat energy developed is in keeping with the wonderful velocities of the bodies; but the rapid cooling is inconsistent with any great masses in collision. A meteor flashing in the sky is the glory of a moment; its velocity is great, but its mass is a speck; all its motion is converted into heat, held for the moment in its feeble grasp, and quickly lost to the pressing throng around it. But the difficulty is not here. Partially arrested motion would be enough to meet the case. And indeed, dealing as we must do with over 500 miles a second of relative motion, it is no great addition to our astonishment to suppose the pair to have already spent an odd hundred miles of their speed in developing heat enough to signal the remainder. A swarm of meteors rushing from us through an advancing gaseous mass would suit our problem admirably, if only the displacements of the bright and dark bands were reversed. This is the obstruction that bars the way to most explanations which would otherwise commend themselves: the lines are shifted the wrong way.

On this account we are apparently forced to admit the presence of two stars in the spot of light known as the Nova, one of them with an atmosphere of intensely incandescent gases giving the bright line radiations and flying from us; the other with a cooler atmosphere absorbing its inner radiations and rushing at us with an equally headlong speed. Each must have derived its excess of heat either from partial loss of kinetic energy, or, as in the Wilsing hypothesis, from the pent-up energy liberated in the stars themselves by their upset of equilibrium at the swing past of the pair. This latter hypothesis, as shown by Dr. Huggins, suits the temporary brilliancy of the Nova, both in the suddenness of its development and in its short duration, better perhaps than any other. But the high velocities, together with their constancy during the six or seven weeks of waning light, offer almost insuperable difficulty in the way of assent to the double-star cause. To account for these we are obliged to suppose that the velocities of the two masses are little influenced by their mutual gravitation; that they are in fact a pair of that class of stars, of which only a half-dozen are known to us, called "runaways"—having high velocities uncontrolled apparently by gravitation. Now to pass over one's own incredulity in such unaccountable velocities, that two of them should be found in the same telescopic field, and both moving in our line of sight, would not be too surprising if there were many such runaway stars; but it passes belief that, amongst so few in all, two should be found on the same path.

Again, the development of heat required for the transient splendour of the star is at the lowest estimate very great; and practically all of this belongs to the bright line star: for the continuous spectrum was feeble, and must be divided between the two stars. We have, therefore, to admit enormous heating effect at the rush-past, not mutual, but one-sided, one of the pair raised to an intense glow, the other comparatively undisturbed. And we have in addition the extraordinary coincidence of the spectrum of the one being, so to speak, the shadow of the other without a common cause.

These considerations naturally throw us back upon a single star hypothesis to look again for some possibility of attributing the apparently double spectrum to a common origin. This was the thought suggested at the first examination of the photographed spectrum at Stonyhurst College. It seemed to be its own evidence of the unity of the light-source; and it claimed more attention when the spectrum was mapped and had been carefully studied. The details of this study are given in full in a paper communicated to the R. A. S. in May last, to be published in the Memoirs. And the sort of disturbance, suggested in the paper to account for the spectrum, may be described more fully here. A spectral line may be widened by the rotary motion of the origin in a plane or planes not greatly inclined to our sight line; for every degree of displacement between two maxima of opposite signs would be given by the foreshortened velocities in the circular motion; and the true position of the line's wavelength would be the middle of its width, if the star were stationary with reference to Earth. It is only necessary, therefore, to consider the conditions under which the blue-side shift of the Nova's lines should present the absorption effect, while the red-side parts show unclouded radiation. A great cyclonic storm of heated gases would produce this double effect, if the heated gases were rushing towards us in the lower depths of the atmosphere, trending upwards, and returning over the stellar limb. In the lower positions the advancing outrush would be screened by a great depth of absorbing atmosphere, while as a high retreating current its radiation would be along a clear line to our spectroscopes.

Disturbances of this nature account satisfactorily for the complicated spectrum of the Nova, if they can be accepted as physically possible. The high velocity presents no formidable objection, when compared with the recorded velocities of similar rushes on the surface of our own much smaller Sun. Father Fenyi, of the Kalocsa Observatory, has recently noted 500 kilometres per second total velocity of a solar prominence eruption. And our very limited knowledge of these disturbances forbids a verdict of impossibility against either the long duration of our supposed cyclonic storm, or the maintenance of its high velocity throughout the cooling process of its rushing gases. The cause of the outburst might still be the tidal upset of equilibrium advocated by Dr. Huggins, but without the inconvenient postulate of the chance meeting of two runaway stars. The pair might be a long period double; they might be crossing our line of sight at any angle; and they might be moving with any velocity.

This interpretation of the spectrum of the Nova is its own reading on the Stonyhurst photographs, inasmuch as the marginal separations of the bright and dark parts of the hydrogen broad lines at F, G′, and *h* are the only similar positions in each that give the correct wave-length intervals F–G′ and G′–*h*: in other words, the true positions of these lines are those which are common to both the bright and the dark parts.

It is true that the photographs are too small to warrant much confidence in the conclusion; but their evidence for it is too striking to be overlooked. On the other hand, it is not confirmed by the Potsdam photographs. On these the intervals F–G′ and G′–*h* are apparently correct when measured from centre to centre of the wide bright lines; but F is only feebly impressed upon the plates. A stronger witness against us is perhaps Professor Young's observation of the hydrogen lines, in the longer spectrum given* by his grating of 20,000 lines to the inch. By this it appears "that the lines were diffuse, like C and F from hydrogen under pressure; but the shading was sensibly symmetrical each way from the middle of the line"*.

This is strong evidence for the centres of the bright lines being the true positions of their wave-lengths, and, consequently, for the separate origin of the dark lines. WALTER SIDGREAVES.

Stonyhurst Observatory, Lancashire.

Luminous Night Clouds.

WE have received from Professor Foerster and Herr O. Jesse, of the Berlin Royal Observatory, a pamphlet on luminous night clouds, and inviting the co-operation of astronomers and others in making the necessary observations. According to the authors the phenomenon has been very noticeable from 1885 until the present time; it is now diminishing in intensity and in two years' time will nearly have disappeared, but within those two years there will be abundant opportunity for observations of great value. The phenomenon is that of cirrus clouds showing bright against the twilight sky. These appear in the latitude of Berlin from May 23 to August 11; in the first few years since 1885 before midnight, but at present generally after midnight. Photographs taken simultaneously at different points around Berlin show that the clouds are no less than 82 kilometres high, so that they receive the Sun's light long after sunset, but become invisible as soon as they fall within the shadow of the Earth. In the morning the phenomenon commences shortly before twilight, and ends when the Sun is less than 8° or 10° below the horizon. It is also only seen close to the horizon and in that part of the sky immediately above the Sun. From frequent observations the authors found that the movements of the clouds at midnight are from the north-east ± 40°, and they have come to the conclusion that the motion is caused by the resistance of the medium surrounding the Earth. In confirmation of this they adduce the fact that the phenomenon has been frequently observed in 50° to 55° south latitude by Herr Stubenrauch at Punta Arenas and several times by officers of ships during the six months following the period of apparition in the latitude of Berlin.

Further, the phenomenon was observed on October 27, 1890, at Grahamstown (33° south latitude) and at Haverford (40° north latitude) on May 17, 1892. These observations point to a motion from north to south and back again. The authors ask for observations of the apparent height of the upper edge of the clouds, for purposes of comparison, in different latitudes. These observations should be taken at intervals of a few minutes with the naked eye, and should be correct within 3 to 6 minutes of arc and 2 to 4 seconds of time. Observations of motion would also be of great value with reference to the determination of the density and of the other conditions of the atmosphere in the upper strata. For observations both of place and motion photography is recommended, the ratio of aperture of lens to focal length being at least 1 to 4. It is, above all, important to observe the time of the passage of the clouds through the equatorial regions; but here the diurnal revolution of the Earth and the movements of the atmosphere will most probably cause a difference between the observed and the calculated periods. The authors speculate on the possibility of the clouds being formed of some gas condensed in consequence of the low temperature, and suggest spectroscopic observations shortly before sunset and shortly after sunrise. The pamphlet concludes with the expression of opinion that the cosmical questions opened up are of sufficient importance to engage the attention of astronomers and geo-physicists, and appeals to observers throughout the world to assist in a work which by its nature is beyond the powers of any single institution. It appears that Herr Jesse has a treatise on the subject in course of preparation.

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* 'Astronomy and Astrophysics,' Apr. 1892.

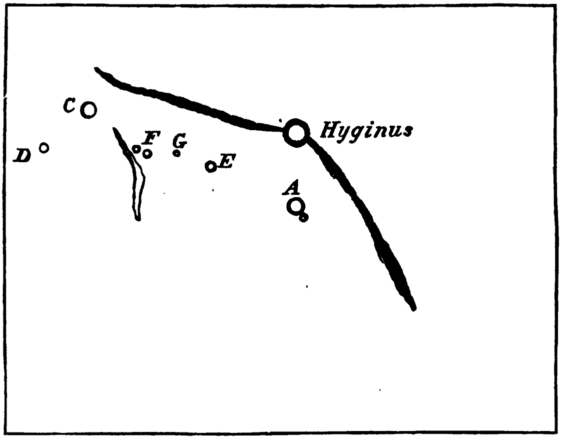
Hyginus N.

So much has been written concerning Hyginus N, that anything further on the subject might, perhaps, be considered superfluous. But, as it seems to be the general impression that this object is a true lunar crater, some investigations that I have recently made may not be uninteresting.

Volume XVII. of the 'Astronomical Register' contains a long article by Neison on Hyginus N, in the course of which he claims that on the night of April 28, 1878, he observed the Moon and saw in the neighbourhood of Hyginus "a great, black, crater-like object" where he had never seen anything of the sort before, although he had often and carefully observed the region. On the same night Mr. Green also observed the Moon, presumably with his 18-inch reflector, and saw the object which Neison observed. His drawing agreed accurately with Neison's, according to Neison's express statement, but in a letter to the 'Astronomical Register' he declared positively that the object was not a crater and could not possibly be a crater. Pratt, too, observed Hyginus N, and said that instead of its being a crater, it was nothing but the shadow of a ridge.

Statements so conflicting as these rendered it doubtful whether we had in Hyginus N a true instance of lunar change. Were the object a real crater, no one after examining all the evidence that has been presented on the subject can doubt that it had only recently come into existence when Klein discovered it. But were it only the shadow of a ridge, the whole matter, as far as lunar change is concerned, is thrown into confusion, and the object may or may not be new.

Under these circumstances, I took the earliest opportunity possible to personally examine the region under suitable illumination. On the night of August 13, 1892, Hyginus was only about twelve hours from the evening terminator, and I directed the 8-inch Clark refractor of Napa College Observatory to the Moon. The night was a reasonably good one (seeing 3 on a scale of 5), but the Moon's altitude was low and the observations were interrupted by fog, which came drifting in, and finally blotted out the sky altogether. I used two eyepieces which gave powers of about 300. I compared the Moon with a map and two drawings of the region of Hyginus which Klein gives in his 'Astronomische Objecte.' In the map (by Klein) objects are laid down in a very faulty manner, which is perfectly inexcusable, as their positions were determined micrometrically by Mr. S. W. Burnham with his 18½-inch Chicago refractor. One of the drawings (made by Neison, April 28, 1878) is very accurate so far as it goes, and is much more useful than Klein's elaborate map. I send you a rough tracing of a portion of Neison's drawing, into which I have inserted some craters I observed, which neither the drawing nor Klein's map show. I have also attached letters to the various craters. In order that this drawing be properly understood, it should be compared with the original. Neison draws the craters C, D, A, E, and one crater in the position F. In this latter position I saw two equal craters as drawn. Between E and F, I saw a little crater which I call G. This was the smallest crater



HYGINUS AND NEIGHBOURHOOD.
Aug. 13, 1892.

picked up. The formation A looked far more like an accidental depression or valley than a true crater, and just outside of its rim I noticed a little crater which was quite an easy object. Indeed, none of the craters seen would be difficult with a similar telescope on a good night, for the objective was not in correct adjustment, and I had no elaborate observing-chair: nothing but a flight of steps. However, the point of all this is that a number of small craters were picked up, while nothing in the least resembling a crater was seen in the position of Hyginus N, and no large crater was *anywhere near* this position. In the place where I expected to see Hyginus N, a whitish discoloration of the surface was apparent, instead of a great, black, crater-like object. It appeared to me that Mr. Green must have been correct in his statement that Hyginus N was not a crater, but I preferred to wait and observe the region at first quarter before deciding.

On the evening of August 29, 1892, the morning terminator was in from 1° W. to 0° longitude. The seeing was about 2 on a scale of 5, and I used powers of 140 and 210 diameters, though the advantage of the higher power over the lower, even during that part of the evening when the seeing was best, was very slight. Of the craters seen before I only saw A, C, and D, D being quite difficult. A fine photograph would show nearly, if not quite, as much detail as I was able to see, but Hyginus N was seen without any trouble. It was apparent at a glance, though, that the thing was not a crater, and that Mr. Green was correct, for it was nothing but the shadow of a ridge. Had it been a crater, and at that distance from the terminator, I should have seen the interior shadow and the illuminated inner wall, and this was the way that D was seen. If a crater is very close to the terminator, and is surrounded by raised walls, it is true it may, if small enough, be seen as a raised mound with a black spot in the centre; but unless an object be seen in one or the other of these ways, it cannot be called a crater.

In order to destroy any doubts as to the accuracy of my observation, I will say that the marking I call Hyginus N is at the same distance from Hyginus as Triesnecker *a*, and a line drawn from N to *a* passes just west of Hyginus. No conspicuous crater was seen in this position or near it. My observations were carefully made, and the telescope used is a good one. For instance, the objective having been approximately adjusted, it showed me on August 27, 10½ hours, P. S. T., the canal in the snow-cap of Mars. This canal was picked up independently in seeing that was not better than Wt. 3. But while my observations prove conclusively that Hyginus N is not a crater, and quite possibly cast doubts upon its being an instance of lunar change, I must not be understood as wishing to throw cold water upon the whole theory that changes are at present in progress on the Moon. In the approximate position W. long. 38° 20′, N. lat. 12° 10′, following Neison's Map I., is a conspicuous crater, which certainly exists, and may possibly have only appeared within recent years. It is pretty well shown in the Lick Observatory photograph of August 31, 1890, and I would be glad to hear from others concerning it.

Napa College Observatory, **Roger Sprague.**
Napa, California, 1892, Sept. 1.

Silvering Glass Mirrors.

The importance of a good reflecting surface in such instruments as the modern silver-on-glass reflecting telescope and the equatorial coudé is obvious. As a rule the silver surface if fairly protected from dust and damp will last many years with but slight loss of light, but must be renewed frequently if the best results are to be obtained. Many different processes and methods of silvering have been from time to time published by different people, and it becomes of some interest to examine these with a view of finding out the particular one that suits certain cases.

Having used reflecting telescopes for many years I have had occasion to try a great number of experiments with a view of getting a good process. It would be tedious to give these in detail, but it may be useful to give some few instances where satisfactory results have been obtained.

The process of depositing the metallic silver on a glass surface is an empirical one; the conditions affecting the reactions are so various that hard-and-fast rules cannot be laid down. The temperature in which the process is carried on seems perhaps the most important thing to be considered, ranging as it may from 35° or 40° to 104° F. according to the reducing agent employed.

I have had occasion to look up the processes published from time to time; some of these are of sufficient interest to be given briefly.

Baron Liebig found in 1835 that on heating aldehyde with an ammoniacal solution of nitrate of silver in a glass vessel a brilliant deposit of metallic silver was deposited on the surface of the glass. To this observation is due the modern process of silvering glass.

The next important step seems to have been taken by Cimeg, who, in 1861, patented a process for silvering mirrors (where of course only the surface against the glass is used) by what has since been known as the Rochelle-salt process. This patent is No. 619, 1861. After cleaning the glass in the usual way he washes the surface with Rochelle-salt solution 1 in 200. For 1 sq. yard of glass he takes 20 grammes of nitrate of silver in solution and adding it to ammonia of commerce till a brown precipitate commences to be produced; to this is added a solution of 14 grammes of Rochelle salts. Using the mixture in this proportion when it becomes turbid he pours it over the glass plate, which has an inclination of 1 in 40, for 30 minutes at a temperature of 68°.

In 1862 Cimeg has another process patented (No. 2314). He uses 20 grammes of Rochelle salts in 300 grammes of water, 20 grammes of nitrate of silver in 15 grammes of water with ammonia to clear; but in place of using a weak solution of Rochelle salts on the surface of the glass before silvering he rubs on the juice of apples, currents, sorbs, or other berries before silvering.

In 1873 Woerther uses glucose as the reducing agent.

In 1876 Pratt patents a process (No. 1259) in which before silvering he treats the glass with 1 part of protochloride of tin in 100 parts water. For large plates he uses 1 part of protochloride of tin, 3 drachms of oxalate of ammonia, ½ lb. putty powder, 4 pints distilled water; this is rubbed on and allowed to dry: he then uses a solution of 2 parts oxalate of ammonia, 4 parts grape-sugar, 1 part lime, 1 part potassic cyanide, in 1000 parts water. In silvering he uses tartaric acid, but does not give details.

There are a few more patents for silvering since the last date of no importance.

In 1881 Piazzi Smyth gives, in 'British Journal of Photography Almanac,' Martin's process in full. This is a pretty well-known process, and in some hands has worked very well. Many other processes, in which the chief variation is the reducing agent employed, have from time to time appeared in the various scientific journals—the most important being that published by Mr. J. A. Brashear in the 'English Mechanic,' vol. 31, p. 327. This is a most excellent process and for ordinary work, when the glass can be put in face downwards, the best I know. This I give further on as I use it.

In the 'Encyclopædia Britannica,' vol. 16, p. 500, two processes, hot and cold, are given; these, though mainly relating to the silvering of ordinary looking-glasses, have a bearing on the process as used for silvering mirrors.

"In the former method there is employed a horizontal double-bottomed metallic table which is heated with steam from 35° to 40° C. The glass to be silvered is cleaned thoroughly with wet whiting, then washed with distilled water and prepared for the silver with a sensitizing solution of tin, which is well rinsed off before it is removed to the silvering table. The table being raised to the proper temperature the glass is laid and the silvering solution at once poured over it, before the heat of the table has time to dry any part of the surface of the glass. The solution used is prepared as follows:—In half a litre of distilled water 100 grammes of nitrate of silver are dissolved; to this is added of liquid ammonia (sp. gr. 0·880) 62 grammes; the mixture is filtered and made up to 8 litres with distilled water, and 7·5 grammes of tartaric acid dissolved in 30 grammes of water are mixed with the solution: about 2·5 litres are poured over the glass for each superficial metre to be silvered. The metal immediately begins to deposit on the glass, which is maintained at about 40° C. (104° F.), and in little more than half an hour a continuous coating of silver is formed. The surface of silver is then cleaned by very carefully wiping with a very soft chamois leather and treated a second time with a solution like the first, but containing a double quantity of tartaric acid. The solution is applied in two portions, and thereafter the glass is once more carefully cleaned of all unattached silver and refuse and removed to a side room for backing up.

"In silvering by the cold process advantage is taken of the power of inverted sugar to reduce the nitrate of silver. This process has been adopted for the silvering of mirrors of astronomical telescopes, notably of Leverrier's great telescope in the Paris Observatory. For ordinary mirror silvering the following is the process recommended by H. E. Benrath:—Two solutions are prepared, the first of which contains the silver salt, the second the sugar preparation. For the silver solution 800 grammes of nitrate of silver and 1200 grammes of nitrate of ammonium are dissolved in 10 litres of water and 1·3 kilos of pure caustic soda in 10 litres of water, and of each of these solutions 1 litre is added to 8 litres of water, which is allowed to rest till the sediment forms and then decanted. The second solution—inverted sugar—is prepared by dissolving 150 grammes of loaf-sugar with 15 grammes of vinegar in 0·5 litre of water, and boiling the solution for half an hour. After cooling it is made up with water to 4200 cubic centimetres. The silvering is done on horizontal tables in a well-lighted and moderately heated apartment, and the glass is cleaned with scrupulous care. For each square centimetre of glass operated on 15 cubic centimetres of the silver solution above described are measured out, and from 7 to 10 per cent. of the solution of inverted sugar is added, both being quickly stirred together and poured rapidly and evenly over the glass. The reduction immediately begins and the solution exhibits tints passing through rose, violet, and black, till in about seven minutes it again becomes transparent and the deposit of metal is complete. This first deposit is extremely thin and allows the transmission of bluish rays. The exhausted solution with floating and unattached dust-like particles of silver is carefully wiped off, the silvered surface washed with distilled water, and again treated with the mixed solutions to the extent of half the quantity used in the first application. The finished surface is wiped and washed in the most thorough manner—for the least trace of caustic soda left would destroy the mirror. The further processes are the same in both methods of silvering."

In Brashear's process, already mentioned, the most important thing is the sugar solution forming the reducing agent. This greatly improves by keeping—a solution that has been made some months being much more effective than a newly-made one. I find it convenient to have always some Winchester quarts of it in stock ready for use. I have for convenience varied his proportions slightly and thus give them as I have found them work so well. For the sugar solution I add to a 10 % solution of loaf-sugar, in distilled water, 10 % of alcohol and ½ % of nitric acid. Solutions of 10 % of nitrate of silver and of caustic potash are separately prepared, the latter one as wanted. These, with sufficient ammonia and a very dilute solution of nitrate of silver, and also a similar very dilute one of ammonia, are prepared, the latter in order to obtain that pale brown colour of the ammoniated solution of nitrate of silver that it is absolutely necessary to have before adding the reducing agent.

Having selected a suitable dish to contain the liquid, in which the mirror can be placed face downwards with about ½ or ¾ inch of liquid underneath, find on the basis of 1 of silver-nitrate solution to 4 of the total required liquid the amount of silver solution needed; to this add ammonia till the first formed precipitate is dissolved, then add one half of this quantity of the potash solution (this is a variation from Mr. Brashear's formula that I have found works well), and again add ammonia till the mixed solution is quite clear, taking care to put in only sufficient ammonia for that purpose; then add the weak solution of nitrate of silver till a clear brown colour is obtained; should this become a dark brown some of the weak solution of ammonia will bring it to a pale brown colour, which must persist if the solution is left standing some time.

The mirror, previously cleaned with nitric acid and distilled water, and suspended in the dish in distilled water of sufficient amount to make up on the addition of the solutions the total liquid required, is lifted out and the prepared solutions mixed with the distilled water and an amount of the reducing solution equal to about one half that of the nitrate of silver solution more or less as the temperature is under or over 60°; as soon as all is intimately mixed the mirror is immersed with one movement, beginning by dipping the edge first and lowering so as to prevent any air-bubbles forming under the glass. In from three to five minutes the silver begins to form on the mirror, the solution changing from pink to dark drown and black, the film thickens quickly, and in from twenty-five to thirty minutes sufficient silver is deposited. The mirror can then be washed and put to soak in distilled water for a few hours, then taken out and dried and polished in the usual way, that is, with a soft pad of clean chamois, and going all over the mirror with light strokes till the bloom is all removed and a fair polish is obtained, finishing with a very little of the finest washed rouge, quite dry, lightly dusted on the pad; it is very important to well consolidate the film of silver by the unrouged pad before using any polishing-powder.

It is a very good plan for any one who is not in the habit of silvering, or to whom the process is strange, to try the proportions of the solutions on some small pieces of glass till a satisfactory proportion for the temperature (for that is the chief factor in varying the amount of reducing solution necessary) of the room in which he is working. The most important thing (after the solutions) is the proper cleansing of the glass, for on the proper preparation of the surface of the glass a very great deal depends.

As already stated, this process is used when the glass to be silvered can be suspended in the liquid; it is not suitable when we attempt to silver surfaces face upwards. The mud formed settles down and prevents any proper deposition of silver; this was a source of considerable trouble when it was required to silver the three-foot mirror, and a pneumatic arrangement was eventually made to hold the mirror by the back, so that it could be silvered face downwards, and up to that size the silvering could be managed.

The great size of the five-foot mirror and its enormous weight (over half a ton without the cell) made it dangerous to suspend it, and the question of silvering became a serious one. In making experiments in order to get rid of the mud formed in the process last mentioned, it was found that by leaving out the potash the silver was deposited from a nearly clear liquid and no mud was formed, and the first five-foot mirror was very successfully silvered in this manner. The solutions of silver and sugar are used in the same proportions without potash, but it is found advisable to use a stronger total mixture. For subsequent silvering of the five-foot mirror the Rochelle-salt process has been used, and this for the deposition of the silver on a surface face up seems to be the best, using, if necessary, two or more applications.

In preparing a large mirror for silvering in this manner it is necessary to form it into a dish by using a band of paraffined brown paper round the edge, standing up an inch or more all round, and mounting the mirror on a swinging support, so that it can be tipped up to throw off the water or spent solutions; in the case of the five-foot mirror, when mounted on the machine this tipping up could be done by the same arrangement used for placing the mirror vertical for testing.

The proportions of solutions used for the five-foot were for each application: 3000 cubic centimetres of silver solution as before ammoniated as already described and 500 c. c. of Rochelle-salt solution, with about 29,000 c. c. of distilled water; this remained on the mirror 28 minutes; another similar application was left on for 30 minutes; after thorough washing, distilled water was left on for some hours and the film dried and polished.

A very fine film of silver was deposited on a five-foot mirror, using one application only of 4000 c. c. of silver solution and 750 c. c. of Rochelle-salt solution: this after one year was found to be in a very good state indeed; this was on the first mirror which, from some defect in the glass, could not be made into a good mirror. The disk of glass was returned to the makers to be replaced by another. I took this opportunity of removing and collecting the whole of the silver by dissolving it in nitric acid.

The assay of the deposit gave a total weight of 26·5 grains of silver on a surface of 2800 square inches, equal to a thickness of $\frac{1}{280,000}$ inch, almost exactly; in actual weight somewhere between that of a threepenny and fourpenny piece, not a large amount of the 400 grammes of nitrate of silver used in depositing the film. The actual waste need not be very much, as the chloride of silver can be easily deposited by the addition of common salt to the spent solutions and the silver thus recovered.

It will be seen that the various processes all have the ammoniated solution of nitrate of silver, and differ only in the reducing agent. The preparation of this solution, in order to get the pale brown colour already spoken of, demands some care. If the solution is too strong, on the addition of ammonia a very flocculent deposit is formed, difficult of redissolution. If after the solution is cleared by the addition of ammonia a strong solution of silver nitrate is added to get this colour, this flocculent deposit occurs; but if the weak solution advised be used, there is not any difficulty in getting the proper colour free from any deposit. This is important. A word of caution may not be out of place concerning the production sometimes of a fulminate of silver, recognized by its dark-grey metallic lustre. This is extremely liable to explode with great violence on the contact of almost any thing; a few drops of water once sufficed to explode some in a beaker and blow it to fragments. By using moderately diluted solutions this danger is obviated. My own experience is not singular in this respect, for Mr. Brashear relates a similar occurrence.

The silver film is not always of the same quality, and experiments are needed to get more information as to what determines the greater density and coherence of some films over others. I have had surfaces of glass silvered experimentally where the film would not wash off with any amount of wet rubbing, these mostly on surfaces that had been silvered many times. Probably the glass in this case was in the best state to receive the new deposit; certainly the condition of the surface does affect the coherence of the silver as well as the amount of the deposit, as judged by the way in which certain parts on a mirror that has been incompletely cleaned show that the deposition has begun long before other parts, necessarily resulting in an unequal thickness of film. With the most careful cleaning of a mirror I have often found that the first application did not succeed, but the second on the surface just cleaned off with nitric acid was all right. The nature of the liquid other than distilled water last in contact with the surface of the mirror seems to be the determining thing.

A. A. Common.

Selenographical Notes.

Arzachel.—This beautiful formation, the most southerly of the magnificent group which includes Ptolemaus, Alphonsus, and

Alpetragius, belongs to the more regular type of the crateriform ring-plains, and is surrounded by a very broad and much terraced barrier, which, according to Mädler, is at no point less than 10,000 feet above the circular and nearly level interior. Neison devotes only a brief space to a description of its more obvious features, to which but scant justice has been done in the few drawings of it which have appeared, though (excepting under a high Sun, when it virtually disappears) it is one of the most striking telescopic objects of its class on the Moon's visible surface. A good 3-inch achromatic suffices, when the ring is not far removed from the terminator, to show some of its more remarkable characteristics, notably the curious structural relation of its north-western border with the south-eastern wall of Alphonsus, the large deep crater A on the western side of the floor, the central mountain, and traces of the terraces on the inner slope of the circumvallation. These last-named features are best seen when the morning terminator has advanced to about 12° east longitude, a phase also very favourable for viewing the detail in the interior.

Under good optical and other conditions the details of the very complex wall of Arzachel afford a most striking example of multifold terraces, extending from the crest to the foot of the inner slope, and associated with 'pockets' (one on the south-east being classed by Schmidt as a cleft) near the summit. They are very correctly shown by Schröter in one of the best of his lunar sketches.

The outer slope, or *glacis*, reminds one of that of Copernicus or Bullialdus and appears to have many circular and oval depressions, without rims, on its surface. Among the craters on the wall, the large crater *b*, associated with the summit ridge on the north-east, is the most conspicuous, and I have often noted a pass or deep valley running down from it to the floor which bears a striking resemblance to that which has been recently recorded on the inner north-eastern wall of Aristoteles. It is not shown by Schmidt or in any of the maps, though it is very obvious under oblique light. It would probably be resolved into a crater-row under the best conditions. A little west of this, below the border crest, are two large craters shown by Mädler and Neison, which also run athwart the inner slope and occupy nearly the whole of its width. Mädler and Neison draw prominently another crater, Δ, on the southern wall, but it is very inconspicuous; I have seen three minute depressions near the site, but have no record of any crater worthy of special mention in the neighbourhood.

The central mountain of Arzachel is a noteworthy object at sunrise. It consists of an oblong mass, rounded towards the north and exhibiting a digitiform outline towards the south. This is due to the presence of three or more spurs or buttresses, separated by deep valleys, which run down to the level of the floor about midway between the centre of the ring and the foot of the south wall. Schröter draws a large deep crater in the place of these spurs; but this probably only represents shadow in the intervening valleys, the true character of which his instrument was not competent to reveal. It may be mentioned, however, that Schmidt records three inosculating craters between two of the mountain forks, the middle one being of considerable size. There is an outlying circular hill of low altitude adjoining the principal mountain mass on the north-west, and a lofty ridge extending from the eastern flank of the latter to inner foot of the south-east wall. The large crater A is of a similar type to Ptolemaus A, being circular and very deep; but it differs from the latter in having a minute craterlet on its south-eastern rim, which I glimpsed at 9^h March 5, 1884, and again at 9^h on April 5 this year, with a power of 350 on my 8½-in. Calver reflector. Schmidt appears to have discovered this object. It does not appear in any other map. The crater between A and the southern end of the central mountain is an easy object, which may be readily detected with a small aperture.

The most remarkable object, however, on the floor of Arzachel is the serpentine cleft, ϕ, on the western section of the interior, between A and the foot of the wall. As a rule all the rills within the lunar walled and ringed-plains are more or less straight, and there is, I believe, no instance of one thus situated which has so winding a course as this. Its commencement on the north is marked by a small crater, which I have seen easily with my 4-in. Cooke achromatic, and, according to Schmidt, there are two craters of a smaller size near its southern end. These I have not succeeded in seeing, nor are they shown by Neison. Mädler does not record this cleft, but shows a valley on its site. The rill, ξ, on the southern side of the floor, is probably an extension of the winding rill towards the south, though I have often endeavoured to trace the connection without success. The southern section of the interior includes much detail of a somewhat doubtful kind. On April 5, 1892, $8^h\ 50^m$ to $9^h\ 50^m$, several craterlets of the smallest class were seen distributed on the floor between the central mountain and the foot of the southern wall. Schmidt shows nothing of these, but draws three craterlets on the northern section of the interior. The interior of Arzachel is very obviously darker on the north than the south, at least under morning illumination: the shading being so graduated as to impress one with the idea that the floor is slightly convex.

A short distance west of Arzachel there are two curious rings (marked *g* and *c* in Neison's Map 23) which are well worth looking at under a low Sun. The more northerly, *c*, is a very perfect saucer-shaped depression with a large central mountain, a deep crater on its northern flank, and an irregular oblong enclosure, *h*, adjoining it on the north. The other ring, *g*, has a still larger crater on its south-western border, and a deep pit, without a rim, on the eastern side of the floor. This is omitted by Mädler and Neison, but shown as a rimmed depression by Schmidt. The rings *g* and *c* are connected by a row of confluent craters of considerable size, which, under a low power or inferior definition, resemble a coarse cleft with raised banks on either side.

Beaumont House, Shakespeare Road,
Bedford, 1892, Sept. 19.

T. Gwyn Elger.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The two last Total Eclipses in Scotland.

Gentlemen,—

It has generally been supposed that the eclipse of 1652, very fairly observed for that time of day, both in Scotland and England, was the last in which a total phase passed over Scotland. There are, however, two others of short duration in the latter part of the same century, both of which were total in the northern parts of that land.

Eclipse of August 12, 1654.—The great eclipse of 1652 (which, according to the maps in Oppolzer's Canon, escaped Scotland altogether in its total phase!) was followed in little more than two years by this one, total about 11 minutes to 9^h in the morning in the neighbourhood of Inverness. As to the partial phase, Mr. William Leybourn, at London, observed the greatest obscuration to be at $9^h\ 4\frac{1}{2}^m$ (morn.), digits $10\frac{1}{4}$. Dr. Wallis at Oxford observed the beginning at $7^h\ 45^m$, the end at $10^h\ 14^m$.

Eclipse of September 23, 1699.—On this occasion there was one of those phenomena, termed by Professor Grant (Monthly Notices R. A. S., Dec. 1871), "slightly total eclipses," formerly termed "total without continuance." At 9^h in the morning the Sun must have been totally obscured across the north of Caithness for about a quarter of a minute only. This, therefore, was the last occasion on which anything in the way of totality could be seen in Scotland. There is a descriptive sheet of this eclipse, in my possession, by John Marr, "professor of navigation," "calculated for the meridian of the famous city of Edinburgh." This gives the greatest obscuration there at $9^h\ 25^m\ 30^s$, digits eclipsed 11. In the appendix to Pepy's diary, there is a letter from Dr. Wallis to S. Pepys, mentioning that his daughter's attention was called to the eclipse by the dimness of the Sun's light while she was writing a letter about 9 o'clock: "It was much like that in the evening a little before sunset."

It is a matter for regret that no observations of these total eclipses of 1654 and 1699 in Scotland are known. Even allowing for the probability of clouds, and the fact of the thinness of the population, it is to be hoped that some old families in the neighbourhoods of Inverness and Thurso might find some mention in their papers.

Faithfully yours,
S. J. Johnson.

Melplash Vicarage, Bridport,
1892, Sept. 8.

Jupiter's Fourth Satellite.

GENTLEMEN,—

As is well known, Sat. IV. of ♃ escapes eclipse for periods of about three years in succession and for alternate periods of three years endures eclipse at every heliocentric conjunction. When there are no eclipses, except near the ends of those periods, there are no transits, occultations, or shadow-passages of this satellite. Our 'Nautical Almanac' conveys no information about it, except that which may be derived from inspection of the daily "configurations" of the planet and its satellites. The 'American Ephemeris' gives, however, the times of the geocentric superior conjunctions of all the satellites. If these could be added, *for the Fourth Satellite only*, to the "Companion to the Observatory," and supplemented possibly by the inferior conjunctions also (supposing the information could be obtained from our Nautical Almanac Office or that at Washington), it would be very much more easy to prepare for a series of measures of position and distance of Sat. IV. at its elongations, and for measures of its deviation from ♃'s centre at conjunctions.

From Mr. Marth's Ephemerides I have calculated a list of superior and inferior conjunctions of Sat. IV. to the end of this year, but, employing only the mean longitude of the satellite, the errors in the times may be as much as one hour, which is of consequence at the conjunctions, though of less importance at the elongations.

Yours faithfully,

Murston Rectory, 1892, Sept. 10. A. FREEMAN.

Observations of the Perseids, 1892.

GENTLEMEN,—

From the 10th till the 12th of August we observed 116 shooting-stars, and marked their paths on three maps, two of which had been copied from Argelander's 'Uranometria,' and the third from Kaiser's 'Hemelplein.' Unfortunately the Moon did not allow us to see the smaller fixed stars, and so prevented us from drawing the shooting-stars' paths more accurately.

On *August* 10*th* we saw forty-nine of them, from $10^h\ 55^m$ (mean time at Utrecht) till $12^h\ 39^m$, and from $13^h\ 1^m$ till $14^h\ 15^m$, *i. e.* about 16 an hour. At $12^h\ 39^m$ the sky grew quite clouded, but at 1 o'clock it was clear again.

Of these forty-nine stars twenty-seven radiated more or less accurately from the point I. (decl. $+54°$, right ascension 4^h), fourteen from II. ($+42°$, $2^h\ 35^m$), three *others* might be referred to either point; nine stars seemed to be scattered at random.

August 11*th*. The weather was somewhat foggy and very damp; a thick dew was rising from the meadows. Of thirty-six shooting-stars observed from $9^h\ 45^m$ till $14^h\ 30^m$ (about eight an hour), fifteen seemed to come from I., nine from III. ($+72°$, $23^h\ 15^m$), and *besides* three might belong to either point; seven did not give a distinct radiant.

August 12*th*. From 10^h 40^m till 15^h 15^m we observed thirty-one shooting-stars (*i. e.* 7 an hour), five of them belonging to IV. ($+55°$, 2^h 52^m), five to V. ($+66°$, 2^h), excluding twelve which might as well be supposed to radiate from IV. as from V.; nine could not be referred to any radiant-point.

Yours truly,
A. A. Nijland,
J. C. Bolt.

Utrecht, 1892, Sept. 12.

Jupiter.

Gentlemen,—

It might interest the readers of the 'Observatory' to know the condition of the Jovian surface during the present favourable apparition, as far as yet indicated by the observers of the Jupiter Section of the "British Astronomical Association."

1st. The Red Spot is very considerably fainter than last apparition, being, in smaller apertures, but a trace of its former self, and requiring moments of best definition to perceive its outlines, which are even then very diffuse. If it be a protuberance on the surface of the planet, its appearance suggests a thicker covering of vapour than heretofore.

2nd. The shoulder, so called, adjacent to the Red Spot is somewhat harder and darker than last apparition, and the south equatorial belt is generally broader and more disturbed than last year.

3rd. The equatorial zone appears to be in greater activity and several white spots are visible in that region, also drifts of dark matter from the north edge of the S. equatorial belt.

4th. The north equatorial belt shows considerable activity, very definite dark spots and markings being visible. Efforts are being made to determine their true position and movements.

5th. The north temperate band is more quiescent than last apparition, still, however, retaining its hue, as nearly as dark as the north tropical belt.

6th. The south temperate band is plainer this apparition, but shows no trace of darker markings so characteristic of last year.

7th. The south polar region is more banded than last apparition.

It is hoped the above notes will stimulate observation of the giant planet of our system.

Yours faithfully,
W. R. Waugh.

Portland, 1892, Aug. 26.

The proposed Memorial to Flamsteed.

Gentlemen,—

Although there can be no doubt of the propriety of the proposed memorial to the first Astronomer Royal in the church of which he was rector during a period of nearly thirty-five years, I must demur to Mr. Tebb's remark that he had "discovered" the register of Flamsteed's burial at Burstow. The late rector, the Rev. T. B. Sikes, showed me the register in question when I first called upon him and saw the church in 1880; and this fact I stated in an article on the Royal Observatory contributed to the Companion to the 'British Almanac' for 1881. Mr. Dunkin afterwards communicated, in a letter published in the 'Observatory' for the latter year, the results of an examination he had made into the circumstance, which seemed so surprising, that no tablet or memorial to Flamsteed had been put up in Burstow Church, by which it appears that Mrs. Flamsteed had left instructions for the erection of one should she not live to have it placed herself, but for some inexplicable reason her instructions were never carried out.

As Mr. Tebb refers to Baily's biography of Flamsteed, I may be permitted here to express the hope, which all astronomers must feel, that that account may shortly be revised or rewritten by some competent hand, correcting Baily's one-sided view of the ever-to-be-regretted difference between Newton and Flamsteed.

Yours faithfully,

Blackheath, 1892, Sept. 17. W. T. LYNN.

OBSERVATORIES.

REALE OSSERVATORIO DI CAPODIMONTE.—The observations made at this observatory and theoretical discussions on the results have been published in two large volumes. The period embraced is from 1863 to 1890, and includes the practical and theoretical works of Nobile, Gasparis, and Fergola. Amongst the numerous papers we notice:—Determinations of Differences of Longitude of Naples—Rome, Naples—Palermo, Naples—Milan; Determinations of latitude and discussions of methods; Determination of R.A.'s of zone-stars; Observations of double stars (Nobile), and a long theoretical paper on double-star orbits of Gasparis; Observations of Comets 1863 IV., 1864 III., 1882 Wells, Great Comet of 1882, Comet Sawerthal; a catalogue of 714 meteors; Meridian observations of Mars and comparison stars in 1879; Determinations of magnetic elements; and a long description of the Reale Osservatorio di Capodimonte.

COLUMBIA (MISSOURI). *Milton Updegraff.*—During 1891 38 observations of comets and small planets were made with the $7\frac{1}{2}$-inch. The latitude of the observatory was determined as $+38° 56' 51''\cdot6 \pm 0''\cdot14$. The computations in connexion with the determination of the difference of longitude (Columbia—Washington) have not yet been completed. The time of the Director is largely taken up with class-teaching in the University.

NOTES.

Mars.—Sir Robert Ball gives in the September 'Fortnightly' a paper on Mars, very well worth reading, particularly that part treating of the possible atmosphere surrounding the planet, founded on a view of the subject recently communicated to the Royal Dublin Society by Dr. G. Johnstone Stoney, F.R.S., where the velocity of the molecules of the different gasses is considered in relation to the power of the attractive force of the planet to keep them from flying off by virture of their own motion, if when at the confines of the atmosphere a free path into space might present itself.

Professor W. H. Pickering at Arequipa has evidently been making some interesting observations, according to communication made by him to the 'New York Herald' of Sept. 1. From the wonderful steadiness of the air there we may expect that some of the best work on the physical appearance of Mars has been done, although he has but 13″ aperture. Of the many absurd things that have appeared in the newspapers concerning Mars, the most elaborate is a prize story called "A Journey to Mars," by J. Pierce, Jun., given in the 'Sunday News, Baltimore,' on Aug. 28.

A Monster Telescope.—A paragraph has been going the round that M. Francois Deloncle, Member of the Chamber of Deputies, has conceived the idea of an enormous telescope to be made in time for the Universal Exhibition of 1900. This telescope is to be a reflector of silvered glass and to be about 10 ft. aperture and some forty-four yards long. There is no reason why, if the money can be found, a telescope as large as this or even larger cannot be made.

Double Stars.—Professor S. W. Burnham gives, in the Ast. Nach. No. 3113–4, a long list of double stars observed and measured with the Lick telescope. Many of these are most difficult objects; some had not been measured since the time of their discovery. Mr. Burnham speaks of a general catalogue he has in hand. We hope that this last list of measures does not conclude his active contribution to this branch of astronomy.

Comet Notes.—Swift's Comet is becoming very faint, but an ephemeris may still be of some use:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
Oct. 3....	0	1	36	46	50	Oct. 15....	23	51	3	42	59
7....	23	57	42	45	40	19....		48	19	41	28
11....		54	11	44	23						

Shortly after going to press last month the discovery of a small comet by Brooks was announced. The following ephemeris is approximate only:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	'		h	m	s	°	'
Oct. 1....	7	33	37	26	11	Oct. 13....	8	16	53	23	40
5....		47	17	25	16	17....		32	57	23	0
9....	8	1	41	24	26						

We reprint the following letters from the 'Times':—

"A MUCH-NEGLECTED ASTRONOMER.

"SIR,—The absence of any memorial in the parish church of Burstow, Surrey, of the Rev. John Flamsteed, for many years its rector and the first Astronomer Royal at the Royal Observatory, Greenwich, has puzzled most modern astronomers who know the great reputation which he had attained during his lifetime as the author of 'Historia Cœlestis Britannica,' a work of which Englishmen may justly be proud, as the foundation of modern observational astronomy, and the precursor of the long series of meridian observations of the Sun, Moon, and planets, on which the most elaborate mathematical theories of their movements of the present day have been based.

"My object in writing this letter, however, is to show that, although there is no visible record in Burstow church or churchyard of any monument in memory of the great astronomer, it was the full intention of his widow, Mrs. Margaret Flamsteed, to provide means for the erection of a memorial after her death. In her will, dated December 3, 1728, is the following clause:—

> "'I further order and direct that my executor (if it please God I doe not live to doe it myself) doe cause to be placed in the aforesaid Chancell of Burstow in Surry, a Marble Stone or Monument with an inscription in Latin, in memory of the late Reverend Mr. John Flamsteed, and I allow twenty five pounds for the expence of it. I would have my own Interrment mentioned in that inscription.'

"Mrs. Flamsteed died in 1730. In her will we have not only positive evidence of the interment of Flamsteed in the chancel of Burstow Church, of which there was at one time much doubt, but we also know that ample funds were provided by the testatrix for the erection of a suitable memorial. Probably the executor deferred the erection of the monument until some convenient period, and then forgot the subject altogether. That he neglected to carry out the exact terms of the will, in which there were no deferred trusts, may be assumed from the following entry dated December 23, 1751, on the margin of the registered copy of Mrs. Flamsteed's will, preserved at Somerset House:—

> "'Administration with will annexed of the goods, chattels, and credits of Margaret Flamsteed, late of East Greenwich, co. Kent, widow, deceased, left unadministered by John Hodgson, the sole executor and residuary legatee, granted to John Tew, the sole executor and residuary legatee named in the will of the said John Hodgson, deceased.'

"Two portraits of Flamsteed are in the possession of the Royal Society. One of these is attributed in the official list, just issued, to Gibson, and was presented in 1785 by Mr. John Belchier, a surgeon, and a Fellow of the Royal Society, 1732 to 1785. The origin of the second portrait is evidently uncertain, but the artist is given in the official list as "Gibson?" It would be interesting to many to know whether either of these can be identified with the two portraits referred to in the following extract from Mrs. Flamsteed's will:—

"'Item, I give to the University of Oxford, to be hung in the Gallery where the Pictures of the Founders of Colleges and other eminent persons hang, a Picture, a three-quarters length, of the late Reverend Mr. Flamsteed, with the frame, drawn by Mr. Gibson in the year 1712, which date it has upon it. . . . Item, I give to the Royall Society, in Crane-court, Fleet-street, London, an Original Picture of the late Mr. John Flamsteed, an half-length, drawn by Mr. Murry, and also give a Picture of Tycho Brahe, a three-quarters length, with each of their respective frames, to be placed where the Pictures of Mr. Boyle and other persons of that learned Society are fixed.'

"The terra-cotta bust of Sir Isaac Newton, referred to in the letter of 'T. E. J.,' in the 'Times' of to-day, is still preserved in the Octagon-room of the Royal Observatory.

"Your obedient servant,

"EDWIN DUNKIN, F.R.S., late Chief Assistant

"Blackheath, S.E., Aug. 24." at the Royal Observatory, Greenwich."

"THE PROPOSED FLAMSTEED MEMORIAL WINDOW.

"SIR,—In your issue of the 20th inst., there is a letter from Mr. William Tebb, of Burstow, as to the obligation resting upon the nation of erecting some memorial to the memory of the Rev. John Flamsteed, who was the first Astronomer Royal for England, appointed by Charles II.—one of the most sensible things that Monarch did—and who held that office for 43 years—viz. from 1676 to 1719. Mr. Tebb gives some interesting particulars of Flamsteed's work, and it must be conceded that a good case has been made out. The proposal is to erect a memorial window, with an appropriate tablet, in the church at Burstow, where Flamsteed officiated as rector, and where his remains now rest in the chancel.

"This object has the sympathy of Mr. W. H. M. Christie, Astronomer Royal, England, and of Sir Robert Ball, Astronomer Royal, Ireland, and it must commend itself to all lovers of the soul-absorbing science of astronomy, of whom it may be said, as of the stars which stud the firmament, that they form a multitude whom no man can number. If the subscription were limited to one guinea each as the *maximum* contribution, a sum equal to the cost of a handsome window may soon be collected.

"Should any of your readers wish to know more of Flamsteed and the benefit not only to science, but to the nation, as the result of one man's work—work accomplished under great disadvantages—(1) from the want of adequate funds, and (2) from a painful affliction which accompanied him to the end of his days—they can gratify that desire by referring to the 'Encyclopædia Britannica' under the head of Astronomy, vol. ii. p. 756, and under the name Flamsteed, vol. ix. p. 289.

"Your former correspondent, a resident of Burstow, regrets that the parish is but sparsely populated and not able in itself to raise the necessary funds. This circumstance need not be mentioned. Flamsteed's memory and the work he performed belong to the nation. The nation has now the opportunity of wiping out a reproach which neither three centuries nor 23 centuries of neglect would succeed in obliterating.

"Surely some official of the Royal Astronomical Society will suggest a committee, fewer in point of number the better, to carry out the object now brought prominently before the public?

"In the meantime, and pending the appointment of such committee, would Mr. Tebb kindly undertake to act as treasurer, *pro tem.*, so that any one interested in this behalf may know where to send his donation whilst the inspiration is upon him?

"Yours faithfully,

"86 Thicket Road, Anerley, S.E." "JNO. PICKERING, F.R.G.S."

"THE PROPOSED MEMORIAL TO JOHN FLAMSTEED, THE FIRST ASTRONOMER ROYAL.

"SIR,—Mr. E. Dunkin (late Chief Assistant at the Royal Observatory, Greenwich), in his letter to the 'Times,' has added interesting details concerning the work and family history of Mr. John Flamsteed, the first Astronomer Royal, which Mr. Pickering has followed up by offering, through your columns, a practical suggestion for carrying into effect the proposed memorial. When in my letter to the 'Times' of August 20 I alluded to Burstow as a large but thinly-populated parish, allow me to say that I did not wish it to be inferred that it would be impossible to raise funds for the memorial in the district, as this would be an unwarrantable reproach upon my neighbours. It is obvious, however, that nearly two centuries of public neglect of a notable scientific Englishman—the father of modern practical astronomy—can hardly at this day fittingly be atoned for by a purely local demonstration. Mr. Pickering has referred your readers for information regarding Flamsteed to the article on Astronomy in the 'Encyclopædia Britannica.' The fullest details will, however, be found in Baily's 'Life of Flamsteed,' a folio volume published in 1835, of which, after failing with booksellers in both England and Scotland and much advertising, I have succeeded in procuring a copy. The 'Penny Cyclopædia' describes this 'as the most remarkable scientific biography of the present century.' No one could read this record of a noble life without feeling that our national neglect was a national disgrace.

"Some of your readers may be interested to hear that, at the suggestion of two distinguished astronomers, the project is to be brought before the Royal Astronomical Society at their first meeting in November; but if in the meantime any students of star-land should feel desirous of co-operating in this undertaking, the Rev. R. E. Snepp, rector of Burstow, Surrey (who has kindly promised to act as treasurer), will be glad to receive their names.

"Yours faithfully,

"Rede Hall, Burstow, Surrey, Sept. 15." "WILLIAM TEBB."

ERRATUM.—No. 191, p. 315, line 4 from bottom, *for* June 21, 1892, *read* June 4, 1892.

THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 194. NOVEMBER. 1892.

CHICAGO ACADEMY OF SCIENCES.

SECTION OF MATHEMATICS AND ASTRONOMY.

THE regular meeting of the Section of Mathematics and Astronomy of the Chicago Academy of Sciences was held at the Dearborn Observatory, Evanston, on Tuesday evening, Oct. 4th, 1892. Prof. G. W. Hough was in the Chair.

The minutes of the last meeting were read and approved.

Mr. S. W. Burnham made some interesting remarks on the new satellite of Jupiter. Mr. Barnard's discovery is by no means to be regarded as an accident, as Jupiter has been watched by this observer for many years. Up to June of the present year, Mr. Barnard has been using the 12-inch telescope of the Lick Observatory in the examination of Jupiter's surface-markings and the phenomena of the satellites. In July he was able for the first time to employ the 36-inch telescope for this work, and a very careful search was made for possible satellites. On Sept. 9th a very small star was seen close to the planet, and as it was at once suspected to be a new satellite, its position with reference to the third satellite was measured. On the two following nights it was reobserved, and no doubt remained as to its true nature. It is described as a much more difficult object than the satellites of Mars, and therefore can probably be seen with only the largest instruments. The period is very nearly $11^h\ 59^m$, and the distance from the planet's centre about 112,000 miles.

Prof. Hough spoke of the importance of the discovery, and of its relation to Galileo's discovery of the first four satellites.

Mr. Burnham added that the claims of the various observers with small telescopes as to their pretended discovery of the satellite were evidently not to be considered for a moment.

The next paper, "On the New Star in Auriga," was read by Dr. Henry Crew, of North-western University. The discovery and earlier observations of the Nova were described, and the various remarkable features of the Nova and its spectrum were pointed out. Prof. Crew's observations were made with the 36-inch telescope of the Lick Observatory. With a single prism the spectrum was very brilliant, but it was very faint with a grating. Attention was called to the peculiar way in which the lines faded away as the star declined in magnitude. Two lines in the red, which were very faint when the Nova was brightest, became brighter as the magnitude decreased, and finally surpassed even C itself. Dr. Crew considers the reappearance of the Nova as a nebula, as distinctly opposed to Mr. Lockyer's meteoric theory. The various other theories were reviewed and commented upon.

Mr. Burnham remarked, in answer to a question as to the position of the Nova, that Mr. Barnard had found by a series of measures that not the slightest change had occurred since the disappearance in the spring. He considers the mode of discovery as offering great encouragement to amateurs having small instruments. An observer with a large telescope depends so exclusively upon the circles in setting, that he loses his familiarity with the sky. The amateur's constant use of star-charts makes it much more likely that he will notice new objects, though many probably escape attention.

Dr. Crew exhibited Prof. Campbell's new map of the Nova's spectrum, and pointed out the chief nebular line.

In speaking of Mr. Barnard's observations of an extremely faint nebulosity now surrounding the Nova, Mr. Burnham expressed his perfect confidence in Mr. Barnard's ability as an observer by stating that he would rather trust Mr. Barnard's observation of a very difficult object than believe in the testimony of his own eye.

Prof. Geo. E. Hale, of the Kenwood Observatory, described an automatic spectro-heliograph recently devised by him. When once adjusted and set in operation the instrument will take photographs of the Sun, showing spots, faculæ, and prominences at any desired interval throughout the day. It is expected that such an instrument will soon be in daily use at the Kenwood Observatory.

Prof. Hale also presented some remarks on a recent communication by M. Deslandres to the Paris Academy of Sciences. In his paper M. Deslandres suggests a method of determining the velocity of the axial rotation of stars. The method depends upon M. Deslandres's statement that the solar faculæ are sometimes sufficiently bright to show the H and K lines reversed in the solar spectrum as photographed with an integrating spectroscope. Prof. Hale criticised M. Deslandres's method, and proposed a means of testing its applicability which will shortly be presented to the Paris Academy of Sciences.

Dr. Crew remarked that he considered M. Deslandres's method disposed of by Prof. Hale's criticism, and thought it hardly necessary that the test be applied.

Prof. Hough made a few remarks on the present appearance of Jupiter and the Red Spot, which is now very faint and may completely disappear. He considers it identical with the spot seen by Cassini to appear and reappear every six years.

The Meeting then adjourned, and the remainder of the evening was spent by the members in observing Jupiter and other objects with the 18½-inch telescope of the Dearborn Observatory.

GEORGE E. HALE, *Recorder.*

THE ASTRONOMICAL SOCIETY OF THE PACIFIC.

Minutes of the Meeting held at the Lick Observatory, September 3, 1892.

President SCHAEBERLE presided.

THE minutes of the last meeting as printed in the 'Publications,' were approved.

A list of presents was read by the Secretary, and the thanks of the Society voted to the givers.

Special attention was called to an enlarged transparency on glass of the Moon (twenty inches square) and an enlarged positive on glass of Jupiter, presented by the Lick Observatory, and to a number of photographs representing the buildings and instruments of the Lick Observatory, presented by Henry E. Mathews.

At a previous meeting of the Board of Directors, Mr. F. McClean, of Tunbridge Wells, England, was elected a Life Member, and 38 other new members were elected.

The Secretary read the names of members duly elected at the meeting of the Directors.

The following papers were presented:—

1. "The Partial Eclipse of the Moon of May 11, 1892," by A. Stanley Williams, F.R.A.S.
2. "The Effect of Parallax on the Phenomena of the Satellites of Mars," by Professor Hussey, of the Stanford University.
3. "Lick Observatory Photographs of Jupiter, 1891," by A. Stanley Williams, F.R.A.S.
4. "Discovery of a New Crater on the Moon-Negatives of the Lick Observatory," by Professor Weinek, of Prague. (Translated by F. R. Ziel.)
5. "Note on the August Meteors of 1892," by Professor Kirkwood, of Riverside.
6. "A Suggestion to University Instructors of Classes in Astronomy," by C. B. Hill, of San Francisco.
7. "Recent Observations of Nova Aurigæ: Comparison of its Present Spectrum with that of February and March," by W. W. Campbell, of the Lick Observatory.
8. "Lick Observatory Observations of Mars in 1892: a Preliminary Discussion," by J. M. Schaeberle, of the Lick Observatory.
9. "Variable Stars of Long Period," by S. D. Townley, Hearst Fellow in Astronomy in the Lick Observatory.

Adjourned.

F. R. ZIEL, *Secretary.*

Jupiter's Fifth Satellite.

Professor Barnard gives in the 'Astronomical Journal' of Oct. 4 (No. 275) an interesting account of his discovery of this satellite, with the measures he made: from the elongations he deduces a period of $11^h\ 49^m{\cdot}63$; but as this is only from measures of the distance obtained between Sept. 9 and 14, it is, of course, subject to a large error. The paper was dated the 17th Sept., and published on the 4th of October, reaching here about the 16th or 17th of October. From the data furnished in this paper Mr. Marth seems to have deduced a period of $11^h\ 57^m{\cdot}12$, while by the 'San Francisco Examiner,' no doubt from an inspired source, the period is given as $11^h\ 59^m$.

Mr. Turner wrote to the 'Times' that he and Mr. Reid, using the Princeton telescope in America, suspected it on the night of Sept. 26, about 10 o'clock, and if Mr. Barnard's period is right this might probably be a real observation. We have not heard that it has been seen even at those favoured observatories to which, according to the newspapers, it was thought worth while to send any information.

It is very much to be regretted that this brilliant discovery, for such it most undoubtedly is, has been communicated to the astronomical world in such a very peculiar way. We have an announcement giving $12^h\ 36^m$ as the period, 112,400 miles as the distance, and 13 as the magnitude, and the Lick telescope as the revealer: this is the first telegram. Then we find that Professor Barnard is the discoverer and that the period is $17^h\ 36^m$,—still without the time of discovery fully stated, from which with either period some idea of its place in its orbit could be found; and for weeks this erroneous time stood uncorrected and no further information was given. Even at the present time, though it is hardly possible that further elongations have not been observed at the Lick Observatory, we have not had the slightest information which would enable even the probable time of an elongation to be given; and as about a hundred revolutions have taken place since the discovery, an error of about one or two minutes would make all the difference. In a fine climate the time that Jupiter is visible would make it possible to re-discover the satellite; but here, where an hour or two of good seeing is quite a rarity, the hopelessness of trying to see such an object by chance is too apparent. Had it been some faint comet we should have had all the information necessary for its full observation in the course of hours or a few days at the most; but in the case of this most important discovery after nearly seven weeks we know very little more than the first telegram gave us.

It is only a short time since an important discovery was communicated by a diffident amateur by means of a post-card: a resort to this method by the Lick authorities would have been preferable to the very inadequate method they have adopted.

A. A. Common.

Two Large Telescopes.

A MONSTER reflecting telescope is projected in Paris, to be ready for the Exhibition it is intended to hold there in 1900. This it is proposed to make of silvered glass.

Another enormous telescope is projected by Professor Pickering, of Harvard College Observatory. This is to be a refractor, and it is proposed to erect it in that earthly paradise for an observer which Professor Pickering has discovered in Peru.

Both of these projects are of interest to the astronomer, and well worth serious consideration, not only from the point of view of the constructor and the difficulties to be met with and overcome in the case of the reflector, but from the hopes that may be entertained, by the astronomer, that much valuable information may be obtained from them. Professor Pickering's circular letter is in so many ways deeply interesting that we reprint it:—

"A LARGE SOUTHERN TELESCOPE.—The wide interest in astronomical research is well illustrated by the frequent gifts of large telescopes to large astronomical observatories by wealthy donors who are not themselves professional students of astronomy. The number of these gifts is continually increasing, and in no department of science has greater liberality been displayed. Unfortunately, the wisdom shown in the selection of good locations for the telescopes has not equalled the generosity with which they have been given. Political or personal reasons, rather than the most favourable atmospheric conditions, have in almost all cases determined the site. These telescopes have been erected near the capitals of countries or near large universities, instead of in places where the meteorological conditions would permit the best results to be obtained. The very conditions of climate which render a country or city great, are often those which are unfavorable to astronomical work. The climate of western Europe and of the eastern portion of the United States is not suited to good astronomical work, and yet these are the very countries where nearly all the largest observatories of the world are situated. The great number of telescopes thus concentrated renders it extremely difficult for a new one to find a useful line of work. The donor may therefore be disappointed to find so small a return for his expenditure, and the opinion has become prevalent that we cannot expect much further progress in astronomy by means of instruments like those now in use. The imperfections of our atmosphere appear to limit our powers, and are more troublesome relatively with a large than with a small telescope. Accordingly, it has not been the policy of the Harvard College Observatory to attempt to obtain a large telescope to be erected in Cambridge. In order to secure the greatest possible scientific return for its expenditures, large pieces of routine work have by preference been undertaken, which could be done with smaller instruments. These conditions are now, however, changed. A station has been established by this Observatory near Arequipa in Peru, at an altitude of more than eight thousand feet. During a large part of the year the sky of Arequipa is nearly cloudless. A telescope having an aperture of thirteen inches has been erected there, and has shown a remarkable degree of steadiness in the atmosphere. Night after night atmospheric conditions prevail which occur only at rare intervals, if ever, in Cambridge. Several of the diffraction rings surrounding the brighter stars are visible, close doubles in which the components are much less than a second apart are readily separated, and powers can be constantly employed which are so high as to be almost useless in Cambridge. In many researches the gain is as great as if the aperture of the instrument was doubled. Another important advantage of this station is that, as it is sixteen degrees south of the Equator, the southern stars are all visible. A few years ago a list was published of all the refracting telescopes having an aperture of 9·8 inches or more ('Sidereal Messenger,' 1884, p. 193). From this it appears that nearly all of the largest telescopes are north of latitude +35°, although this region covers but little more than one-fifth of the entire surface of the earth. None of the seventeen largest and but one of the fifty-three largest telescopes are south of this region. Of the entire list of seventy-four, but four, having diameters of 15, 11, 10, and 10 inches, are south of +35°. The four largest telescopes north of +35° have apertures of 36, 30, 29, and 27 inches respectively. But few telescopes of the largest size have been erected since this list was prepared, and the proportion north and south is still about the same. It therefore appears that about one quarter of the entire sky is either invisible to, or so low that it cannot be advantageously observed by, any large telescope. The Magellanic clouds, the great clusters in Centaurus, Tucana, and Dorado, the variable star η Argus, and the dense portions of the Milky Way, in Scorpius, Argo, and Crux, are included in this neglected region. Moreover, the planet Mars, when nearest the Earth, is always far south. The study of the surface of this and of the other planets is greatly impeded by the unsteadiness of the air at most of the existing observatories. Even under the most favorable circumstances startling discoveries—relating, for example, to the existence of inhabitants in the planets—are not to be expected. Still, it is believed that in no other way are we so likely to add to our knowledge of planetary detail as by the plan here proposed. The great aperture and focal length and the steadiness of the air will permit unusually high magnifying-powers to be employed, and will give this instrument corresponding advantages in many directions,—for instance, in micrometric measures, especially of faint objects. It can be used equally for visual and photographic purposes, and in photographing clusters, small nebulæ, double stars, the Moon, and the planets, it will have unequalled advantages.

"A series of telescopes of the largest size (including four of the six largest, the telescopes of the Lick, Pulkowa, U. S. Naval, and McCormick observatories) has been successfully constructed by the firm of Alvan Clark and Sons. But one member of the firm now survives, Mr. Alvan G. Clark; and he expresses a doubt whether he would be ready to undertake the construction of more than one large telescope in the future. The glass is obtained with difficulty and often only after a delay of years. A pair of discs of excellent glass suitable for a telescope having an aperture of forty inches has been cast, and can now probably be purchased at cost, $16,000. The expense of grinding and mounting would be $92,000. A suitable building would cost at least $40,000. If the sum of $200,000 could be provided it would permit the construction of this telescope, its erection in Peru, and the means of keeping it at work for several years. Subsequently, the other funds of this Observatory would secure its permanent employment. Since a station is already established by this Observatory in Peru, a great saving could be effected in supervision and similar expenses, which otherwise would render a much larger outlay necessary.

"An opportunity is thus offered to a donor to have his name permanently attached to a refracting telescope which besides being the largest in the world would be more favourably situated than almost any other, and would have a field of work comparatively new. The numerous gifts to this Observatory by residents of Boston and its vicinity prevent the request for a general subscription; but it is believed that if the matter is properly presented, some wealthy person may be found who would gladly make the requisite gift, in view of the strong probability that it will lead to a great advance in our knowledge of the heavenly bodies. Any one interested in this plan is invited to address the undersigned.

"Edward C. Pickering,
Director of the Observatory of Harvard College."

"Cambridge, Mass., U.S.A.,
September, 1892."

This circular pretty clearly explains all about this telescope. The difficulties of construction of the Lick telescope have been so successfully overcome, that there need be no fear that this one, which would probably be built on the same lines, would be equally successful in this respect.

Of the propriety of the appeal which Professor Pickering makes there can be no doubt. It is very rarely that one can be made on such absolutely good grounds as this rests upon. From such an instrument in such a climate, under such admirable direction, most valuable and interesting results may with certainty be looked for—results of great importance to the astronomer, and which will be a source of great gratification to the donor of such a happily favoured telescope. I sincerely hope that Professor Pickering will be successful in his appeal to his fellow countrymen.

With regard to the Paris telescope, the large size of the projected telescope over anything yet done makes the question of construction much more interesting. The largest silver-on-glass mirror yet made is my own 5-ft. From this to 10-ft. is a great step; from a focal length of 27 feet to 140 is a great leap. It brings us face to face with enormous difficulties, not perhaps insuperable if met in the proper way, but which may very easily become so.

I propose to briefly consider some of these and make a suggestion as to the best way to deal with them. Taking it for granted that the telescope, if properly made, will give results in light grasping power and definition in proportion to its size when compared with smaller instruments, and leaving out of consideration entirely the question of the suitability of the neighbourhood of a large city as the site of a big telescope, with the remark that while I admit that the climate may not be the best possible, I should expect to gain in any given place by increasing the size and power of the telescope used whatever might be the advantages elsewhere.

As far as can be gathered from the English newspapers, the projected telescope is to have an aperture of about 10 feet and a focal length of about 140 feet—truly a gigantic instrument! Nothing has been said definitely about the style of mounting, but it is clearly meant that many observers shall be permitted to use the telescope. This last condition is of vital importance and affects the question greatly, for if the right kind of mounting is used to gain this end the whole thing becomes a feasible matter.

If it is intended, as I gather from what appears in this morning's 'Standard,' to mount the telescope as an equatorial, with a cupola in the usual style, then I have no hesitation in saying that however well such an arrangement might do for one observer doing delicate astronomical work, it would be entirely unsuitable for giving many observers an opportunity of seeing any celestial object; to do this properly it will be necessary to resort to some modification of the altazimuth mounting, with the eyepiece coming through the trunnions on which the tube of the telescope rests.

The unsuitability of the ordinary form of equatorial mounting becomes apparent when we remember that in order to render the eyepiece available for any position slightly off the meridian the upper part must be rotated, and this in such a large instrument would have to be frequently done as the telescope moved in right ascension; then the observer would have to be carried round at an irregular rate at a height of some 150 feet on some sort of stage, that while safe enough for one or two skilful observers could never be used by the world at large.

By adopting some form of the altazimuth mountings already so well known, with suitable modifications, all these difficulties could be overcome and many advantages gained.

A. A. Common.

[To be continued.]

Potsdam Observations of Motion in the Line of Sight.

THE first part of the seventh volume of the Potsdam Observations, which has recently appeared, is one of special importance, for it contains the details of the spectrographic determinations of the motions of stars in the line of sight, which have been so successfully carried on during the past three years at the above Observatory. The peculiar merit of these determinations lies in the great advance which has been made by Drs. Vogel and Scheiner in the accord of separate observations, an advance attributable chiefly to the substitution of photography for direct eye observation and to the extreme care given to making the spectroscope absolutely rigid. This improved accord has resulted in two very important discoveries—a sufficient harvest from three years' work, even if they stood alone—the recognition of the orbital motion of Algol and of that of Spica. And, in addition, some 49 other stars have had their motion of recession or approach ascertained within much closer limits than had been previously accomplished.

Much of the information given us in the present volume has of course been published by Prof. Vogel before—the description of the spectroscope, for example, the methods of measuring the photographs, and the mean results for the various stars observed,—so that there is no need now to notice them at length. But a very great interest attaches to the remarks Prof. Vogel makes on the work of other labourers in the same field and to the comparison which he draws between their results.

The first point to be noted is that the photographic method has four great advantages over that of direct eye observation: the photographic plate is not affected by those atmospheric tremors which try, and perhaps often deceive, the eye so much; it is easy to compare many lines at the same time; and the photograph remains for future reference, the actual measures are made at leisure, and can be repeated any number of times for the sake of check. The fourth point is that by prolonging the exposure a record may be obtained of even a somewhat faint spectrum. So that though the Potsdam refractor has an aperture of not quite 12 inches, yet its results have compared closely as to accuracy with those obtained directly by Prof. Keeler with the great refractor of Mt. Hamilton. This being so, it follows that eye observations made with apertures practically equal to that of the Potsdam telescope might reasonably be expected to be much rougher, inasmuch as the observer would often be working at the very limit of vision.

Prof. Vogel criticises at some length the method of obtaining the comparison spectrum employed at Greenwich. I do not think, however, that this was a really weak point in the Greenwich apparatus, nor the Potsdam method the real reason for the success obtained there. A point Prof. Vogel might have criticized with greater reason is the systematic error which appears to affect the Greenwich measures, depending, it would seem, upon the position-angle of the slit and consequently of the spectrum, and which may be due either to slight flexure of the apparatus or to personality. It is not clear whether a similar possible cause of error has received attention at Potsdam. There can be no doubt that the chief source of the accordance of the Potsdam results lies in the great care which has been taken to secure stability, but the arrangement of the vacuum-tube does not appear to us to be theoretically correct. No image of the tube is formed on the slit, and the arrangement assumes the collimation of the spectroscope to be invariable. Probably with so stiff a mounting this was practically the case; but a similar arrangement would not have been admissible under the Greenwich conditions. Prof. Vogel points out correctly enough that it is of first importance in the Greenwich arrangement that the image of the vacuum-tube should fill the slit. This was sufficiently provided for, the image being some five or six times as broad as the slit opening, so that a very considerable shift would be needed before one or other of the jaws of the slit would be imperfectly illuminated. And the image of the tube being very sharply focussed on the slit, such a change would be at once detected by the change in the appearance of one edge of the line. Again, the parallel arrangement of the tube was decidedly preferable where eye observations had to be made, as a line of sufficient length for accurate measurement was secured; the perpendicular arrangement was demanded by the Potsdam method, in which no image is formed on the slit, a method which has the serious drawback that the two spectra, that of the star and of the artificial source of light, are superposed. No doubt the ingenious and careful methods used in the measurement of the plates have overcome, in a great degree, the effect of this confusion, and Prof. Vogel can urge his success as justifying his arrangement. Still the drawback appears a real one.

The comparison of results which Prof. Vogel institutes is principally with Greenwich. Mr. Seabroke's measures, though most perseveringly and carefully made, were evidently attempted with wholly insufficient means; and Dr. Huggins unfortunately laid down the work twenty years ago, which he was the first to devise and undertake, so that his results are all separated by a wide interval of time from those of the present volume. He has not either published the details of his observations, so that no materials are available for determining probable error. The Greenwich results, on the other hand, have been carried on for 17 years and published in the fullest detail, so that they are open to the most minute investigation.

The first result which comes from a comparison of the three series of measures is as follows:—28 stars are common to the three, and of these, one, Spica, shows orbital motion rendering it unsuitable for purposes of comparison. Of the remaining 27, all three observatories agree as to the direction of the motion in twelve cases. Dr. Huggins stands alone in seven cases, the Potsdam observations stand alone in eight. In no case does the Greenwich result stand out in contradiction to both the others. Further, not only did the Greenwich observations anticipate in seven cases out of fifteen the change of sign which the Potsdam observations show as compared with those of Dr. Huggins, but in five of the remaining eight they brought the large motion found by Dr. Huggins down to a much smaller one. In the three outstanding cases Dr. Huggins did not estimate the rate of motion but only its direction, and it is therefore impossible to say whether the Greenwich value would have been greater or less than his.

The following Tables show how the mean results of the three Observatories compare for the stars observed in common. The motions are expressed in German geographical miles per second, and are taken from the Potsdam volume.

I. Stars for which the three Observatories are in agreement.

	Dr. Huggins.	Greenwich.	Potsdam.
Betelgeuse	+ 4·8	+4·1	+2·3
Rigel	+ 3·3	+4·2	+2·2
α Coronæ	+	+2·7	+4·3
Capella	+	+5·1	+3·3
Aldebaran	+	+6·7	+6·5
Arcturus	−12·0	−9·7	−1·0
Vega	−10·6	−7·5	−2·1
α Cygni	− 8·5	−8·3	−1·1
α Ursæ Majoris	−11·5	−7·3	−1·6
γ Leonis	−	−3·9	−5·2
ε Boötis	−	−2·0	−2·2
γ Cygni	−	−3·3	−0·9

II. Stars for which Dr. Huggins stands alone.

	Dr. Huggins.	Greenwich.	Potsdam.
Sirius	+4·5	−1·8	−2·1
Regulus	+3·2	−0·4	−1·2
ε Ursæ Majoris	+4·1	−0·6	−4·1
β Leonis	+3	−1·8	−1·6
δ Leonis	+3	−3·0	−1·9
η Ursæ Majoris	+	−1·7	−3·5
γ Cassiopeiæ	+	−1·7	−0·5

III. Stars for which Potsdam stands alone.

	Dr. Huggins.	Greenwich.	Potsdam.
Castor	+ 5·5	+0·7	−4
β Ursæ Majoris ..	+ 4·1	+1·9	−4·0
γ Ursæ Majoris ..	+ 4·1	+2·1	−3·6
ζ Ursæ Majoris ..	+ 4·1	+1·3	−4·2
Procyon	+	+0·8	−1·2
Pollux	−10·6	−7·4	+0·2
α Pegasi	−	−5·1	+0·2
α Andromedæ	−	−6·0	+0·6

The above comparisons certainly suggest systematic differences between the different observers and between the two systems of observation. But this is not the only possible explanation of the differences between the three columns in Tables II. and III. In spite of the wide differences between the individual Greenwich measures, the mean results for lengthened periods appear to point, in the case of some stars, to a progressive change of motion; so that, accepting the Potsdam results as accurate to-day, those of Dr. Huggins and of Greenwich may also have been correct when obtained. Table IV. gives a comparison for 7 out of the above 15 stars. It should be added that three stars well observed at Greenwich, viz. α Andromedæ, Pollux, and α Pegasi, show no such progressive change.

IV. Greenwich stars showing progressive change.

	Dr. Huggins.	Greenwich.			Potsdam,
	1872.	1875–80.	1881–85.	1886–90.	1888–91.
Sirius	+4·5	+5·5	−2·6	−3·5	−2·1
Castor	+5·5	+2·5	+0·8	−0·1	−4
Regulus	+3·2	+5·4	+0·0	−1·1	−1·2
Procyon	+	+5·4	+1·6	−1·9	−1·2
β Ursæ Maj. ..	+4·1	+5·4	−1·2	−6·5	−4·0
ε Ursæ Maj. ..	+4·1	+3·9	−1·1	−7·2	−4·1
γ Cassiopeiæ ..	+	−5·6	−3·2	+0·4	−0·5

Prof. Vogel himself refers to the case of Sirius, but sets aside the evidence for change of motion. In this connection it may be of interest to cite Prof. Vogel's own earlier observations made at Bothkamp by the visual method. The following table shows the results obtained for the stars observed at the four Observatories:—

	Tulse Hill.	Bothkamp.	Greenwich.	Potsdam.
Sirius	+ 4·5	+ 9·4	−1·8	−2·1
Procyon......	+	+13·0	+0·8	−1·2
Capella	+	+ 3·5	+5·1	+3·3
Altair........	−	−10·4	−6·6	−5·0
Vega	−10·6	−11·2	−7·5	−2·1

The comparison of course, as it depends on so few observations, is very insufficient, but the close correspondence of the Bothkamp observations with those made nearly contemporaneously by Dr. Huggins is certainly striking, especially when taken in connection with the tendency of the Greenwich results, intermediate in point of time, to be intermediate in value between the early observations of Dr. Huggins and these recent measures of Profs. Vogel and Scheiner.

The comparison of forty-eight stars observed both at Greenwich and Potsdam gives an agreement of sign in thirty cases; and in the eighteen cases of discordance the later Greenwich results in several instances differ from the earlier, and in the direction of greater agreement with those just published at Potsdam.

The point Prof. Vogel lays most stress upon in his criticism of the Greenwich observations is the wide range of the individual measures, which he illustrates by a table of extreme results. On this point Dr. Scheiner dwelt with even greater emphasis in his 'Spectralanalyse der Gestirne.'

Now though unquestionably it is rather severe upon an observer in any field to exhibit in parallel columns the extreme results obtained in the course of seventeen years' work, yet no objection can be taken to it if the conditions of the observations be clearly set forward at the same time; though possibly Prof. Vogel himself might think it a little hard, if the above comparison between his Bothkamp and Potsdam measures were set forward as a gauge of the quality of his observations. Yet that would be a comparison between *means* of measures, not between *extremes*.

The first point to be borne in mind is that the rule at Greenwich is to publish *everything*, and that it was especially needful in a new research of this kind to give to the world each observation precisely as it stood, without cancelling or altering a single figure. A moment's thought will show how necessary this was in this particular case, and yet what observer but will feel how little he would like to apply the same rigidity to all his own observations! And it needs but a short inspection of the Greenwich results to see that the elimination of but a comparatively small proportion of observations, many expressly noted at the time as having been made with extreme difficulty, would make a great change in their general appearance of agreement. It must be further remembered that for the most part there was but a single observer available for the work, and that but a small portion of his time could be given to it. It was impossible, therefore, to pick and choose the most favourable occasions; a night of bad definition must be utilized as far as possible; and, on the other hand, other duties forbade an excellent night being used to the uttermost. When it is considered that with the dispersion used many of the spectra were near the very limit of visibility, and that the great majority of the observations were obliged to be made upon the broad and diffused F line of first-type stars, no wonder can be felt that the probable error runs high. As Prof. Vogel shows, it is about nine times as great as his own. The latter would appear to be about equal to the probable error of the observations obtained by Prof. Keeler with the great Lick telescope, who used a dispersion and a light-gathering power about nine or ten times as great as that in use at Greenwich, whilst systematic differences between Potsdam and Greenwich may in some cases be due to the fact that the measures were made in entirely different regions of the spectrum.

The Greenwich "probable error" is therefore in just the same proportion to the Lick as we should reasonably expect from the difference in the instrumental power employed. But that by the most skilful use of photography in the place of direct eye observation Prof. Vogel should have raised the accordance of his observations, made with a slightly smaller aperture than that at Greenwich, to the level of those made with the great 36-inch telescope of Mt. Hamilton, is a great achievement indeed, and it has undoubtedly put this entire branch of astronomical research, one of the most important and promising available to us, upon a higher and more satisfactory footing than it has ever been before. E. W. MAUNDER.

Jupiter in 1892.

ON six nights recently, the 8-inch Clark refractor, of Napa College Observatory, has been turned on Jupiter for physical observations, under atmospherical conditions which, though never perfect, were reasonably satisfactory. The observations have furnished a fairly good index to Jupiter's general appearance at the present opposition. The bright equatorial region does not seem to contain any of the conspicuous transverse dark streaks observed last year, but on two nights a narrow faint belt was seen dividing it centrally. This belt was traced clear across the planet's disk at $19^h\ 45^m$, Oct. 6. The whole equatorial region presented a very broken and complex appearance at the time, but no well-defined markings, except this belt, were seen. The belt was again seen at 19^h, Oct. 7, when it was traced from the preceding limb to a point a little following the planet's central meridian.

North of the equatorial region are two conspicuous belts, and a third which is very narrow. The term "belt" of course refers only to a dark region. No markings have been seen in the first N. belt, except from $18^h\ 30^m$ to 19^h, Sept. 21, when my notes mention a bright streak traversing this belt from the preceding to the following limb. But I have not been able to see this streak a second time. At the time this streak was seen another was seen in the second N. belt. This one is much easier to observe, but it gradually draws closer to the northern edge of the belt, so that about 3½ hours after the transit of the Great Red Spot it can no longer be seen. No black spots have been seen on the southern edge of this belt, and probably none such exist, but a black spot just within the northern edge transited at $19^h\ 52^m$, Sept. 30. This spot is nothing but a place where the space between the streak and the northern edge of the belt is more conspicuous than elsewhere. The third N. belt is usually easy, and has been traced clear around the planet, though one part of it is very difficult. My notes state that at $21^h\ 47^m$, Sept. 30, this belt was seen crossing the entire disk, "but it was very difficult." There is a little black spot on the belt. It crossed the central meridian at $21^h\ 12^m$, Sept. 26. This belt is bordered on the N. by a bright region at least twice as broad as the belt itself. This bright region was well seen on two nights, and is bordered on the N. by the dark polar cap. As there is considerable contrast between the bright region and the polar cap, an appearance somewhat similar to that of a belt is sometimes produced under a low power, but I can definitely state that no such belt exists.

The planet's southern hemisphere is much more interesting than the northern. It too contains three belts, the first of which is the most conspicuous on the planet. When the Great Red Spot is about an hour past the central meridian, a bright streak can be seen traversing this belt lengthwise, and certainly south of the middle of the belt. Jutting out from the bright equatorial region into this belt are two bright prominences. Their S. ends lie in the middle of this belt, and streaming away from each of these ends is a bright streamer. The first streamer runs into the second prominence, while the second streams away for a long distance, and in the end probably runs into the bright region south of the belt. These prominences were well seen on the night of Sept. 24, when the longitude of the point halfway between them was estimated to be the same as that of the black spot on the third N. belt. At $16^h\ 21^m$, Oct. 7, a bright spot in the first S. belt crossed the central meridian. This adjoins the north edge of the belt, and just precedes a much darker portion of the belt, which is only about half as wide as the whole belt. As the Red Spot comes into sight, the fainter or southern portion of this belt disappears, but a narrow belt comes into view, which has about the latitude the S. edge of the first S. belt should have did it retain its original width. At 19^h, Oct. 7, this narrow belt was seen extending from the preceding limb nearly to the Red Spot.

No satisfactory view of the Red Spot has been obtained with our telescope, owing to the presence of strong moonlight or bad definition when it was on the central meridian. It can be seen pretty readily, however, that the surface is of a somewhat different colour where it lies than in the surrounding bright region, and the outline of the following end has been seen, but never the complete outline. There can be no doubt that it precedes Marth's Ephemeris. Prof. Hough informs me that the difference is about twelve minutes.

The second S. belt is rather faint and has three bright streaks in it. All three of these were satisfactorily seen to be completely isolated from the sides of the belt. One of these has a white spot about twice as long as it is broad at its following end. This spot transited at 21ʰ 59ᵐ, Sept. 30. This spot was also observed Sept. 26, when the third S. belt was seen to come to an abrupt ending in just about the same longitude, and the southern edge of the second S. belt ended a little following. This belt is not uniform in width, but in one place becomes very diffused and swollen on its S. side for a while. Just where the S. edge of the belt should come, did it remain its original width, are three very plain white spots of about uniform size. Each of these has a smaller companion on its south side. The companion to the second (at least this was the case when they were first observed, Sept. 24) was the most difficult, and was not exactly south of its primary, but their position-angle as referred to Jupiter's N. pole was about 155°. These pairs of spots follow each other at intervals of about 35ᵐ. The first pair transited at 19ʰ 50ᵐ, Sept. 26; 22ʰ 13ᵐ, Sept. 30. Following this system of spots, the belt resumes its original width. The second of the bright streaks within the belt has a length equal to about one-third the apparent length of the belt, and its preceding end just follows the third pair of white spots. In the bright region, and just on the south edge of the belt, is a bright spot, which follows the following end of this bright streak by a very few minutes. The third bright streak transited at 17ʰ 53ᵐ, Oct. 7, and has a length about equal to that of the second. It also is followed by a bright spot on the southern edge of the belt. The bright region seems to be hollowed out to receive this spot. This belt is not equally dark throughout, the part south of the Red Spot being the darkest.

The third S. belt is, as said before, not complete, nor is it of equal intensity throughout. At 19ʰ 16ᵐ, Oct. 7, when a conspicuous black spot in this belt was on the central meridian, the belt itself was only strongly suspected. But the part that follows the Red Spot is very easy. In the latitude of this belt is a very large, faint black spot. This spot transited at 19ʰ 51ᵐ, Oct. 6, when a little white spot that is on the preceding end of the large spot was very satisfactorily seen. This little spot is the smallest spot I have observed on Jupiter yet. The large spot itself reminds me very much of the "new red spot" of last year.

All times are in G. M. T. ROGER SPRAGUE.

Napa College Observatory,
Napa, California, Oct. 10, 1892.

Some Longitude Trips.

[Continued from page 361.]

It was determined to begin operations in the present year for the Montreal longitude when the ice had broken up sufficiently to make the journey to Canso comfortable, and the first night of observation was April 19. Mr. Klotz, of the Canadian Government Survey, who had been deputed to assist Prof. McLeod, was stationed at Montreal; Prof. McLeod himself was at Canso, I was at Waterville, and Mr. Hollis remained at Greenwich. We were to remain thus for a "set of three fine nights," *i. e.*, until each station had secured at least three good nights of observation; and then Prof. McLeod was to exchange with Mr. Klotz, and Mr. Hollis with myself, for a similar set of three nights. The operations were then to be intermitted until the autumn, to allow time for many important matters which must intervene, such as the Paris Greenwich longitude, the interchange of two observers across the Atlantic, and the Greenwich Visitation Day.

A little explanation of the "set of three fine nights" is perhaps advisable. In most longitude work there are only two stations involved; it is customary to work only when the sky is clear at both stations, so that the comparison of the two clocks by telegraph is made nearly at the same time as the determination of the error of each clock by stellar observations, and an approximate knowledge of the clock-rate is all that is necessary. This method is imperative when the clock has a variable or uncertain rate, owing to exposure at remote stations or the disturbance caused by transport thither. But when there are facilities for establishing a good clock, some time before the operations, in a room kept at constant temperature, the clock-rate can be depended on for 24 hours (or perhaps longer), at least as certainly as a good observer's personal equation, and the importance of simultaneity for the observations at different stations is greatly diminished. It is to be remarked, moreover, that the telegraphic comparison of the clocks affords a valuable check on the rates,—the more valuable as the number of stations is increased. And, in brief, the general view taken of the operations in the present instance was as follows:—We had four clocks, M, C, W, G (at Montreal, Canso, Waterville, Greenwich), regularly compared by telegraph at least once a day (M, C and W, G twice); the going of one of these, G, is known independently from the regular Greenwich transit circle observations, and the rates of all the others can be deduced; the going of any clock can then be laid down as a curve with an arbitrary zero, which will be completely known when the error at any one epoch is known; and the observations of any observer on any night at (say) Canso give one determination of the arbitrary zero for C.

Now the following is a diagrammatic account of the course of the work for the third part of the observations. A cross represents a "half-night" and a double cross a full night.

	G.	W.	C.	M.
Aug. 16	× ×	× ×	o	o
17	o	o	o	×
18	×	× ×	o	× ×
19	o	×	o	o
20	× ×	o	o	× ×
22	× ×	o	× ×	× ×
23	×	o	× ×	× ×
24	o	×	× ×	o
25	×	×	o	o
26	× ×	o	×	× ×
28	× ×	o	o	× ×
29	× ×	o	×	× ×
30	× ×	o	× ×	× ×

It will be seen that there were three good nights at Greenwich and Montreal by August 22; but not their equivalent at Waterville or Canso till August 24, and even then the distribution of the nights made it advisable to continue the series a few days longer.

I left for Waterville on April 13, 1892. At that time one could get no further than Killarney by rail, though a railroad extension to Caherciveen was in course of construction. The completion of this extension brings Waterville within ten miles of eastern civilization, but I had to drive forty from Killarney. The instruments went on a heavy cart at a leisurely pace, and by travelling all night got there before me, for I stayed the night in Killarney and spent a most peaceful evening by the lake side, looking at the Moon and a brush fire away on the mountain. The forty-mile drive next day on an Irish side-car was not so peaceful, though nearly as uneventful, and with just as lovely scenery for surroundings. The chief incident of the drive was the halt for lunch at "the forge," which was nothing more elaborate in the way of a hostelry than the name literally signifies. We had all brought our lunch with us—the driver, the horse, and myself: the horse was invited into the forge and served with a chair to support his nose-bag; the driver and I munched outside, and admired the deftness with which the smith's son, a lad with a beautiful Greek face, made straight nails out of crooked.

When we had all finished we set out to climb "the Ballough." There is a hill to climb whether one goes to Waterville or to Valentia, as we learnt from them who of old time went to Valentia for longitude purposes. This is, in fact, an old legend of how Mr. C. and Mr. D. were seated in a cart climbing this hill, when the joy of the horse on reaching the actual or fancied summit caused it to start forward and leave Mr. D. and a barometer in the road. Remembering this, I walked up the hill and down the other side and in due course arrived at the "Butler's Arms" of Waterville, where Mrs. Mac (the rest of whose name is only used on state occasions) provides the best of accommodation for man, beast, and angler. The anglers were having terribly bad luck at the time, and I witnessed such a continuous cheerfulness under trying circumstances as convinced me that I could never, never be a salmon-fisher. Fancy spending a holiday of a fortnight in throwing flies for twelve hours a day without a rise the whole time, and then claiming to have enjoyed it thoroughly! which is why I distinguish between anglers and men in describing Mrs. Mac's guests.

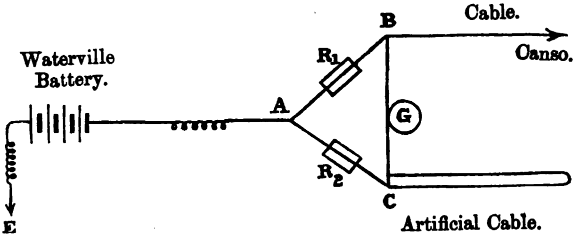
The chief industries of Waterville are, of course, telegraphing and fishing. The former engages the attention of about half the population, and the latter of the whole. The telegraph station consists of a series of low buildings, the chief of which contains the operating-room—an office which never closes day or night all the year round, and where the hours after midnight are indeed particularly busy ones. Mr. T. J. Wilmot, the superintendent, is one of the busiest of men; but though he was installing the electric light at the time I was there, and had many other claims on his attention, such as the repairing of one of the cables, he found time to attend to my smallest wants, even with alacrity. But then a busy man always has plenty of time. Concerning that same electric light plant, when the cart came along the road with the heavy machinery the geological formation of Kerry was made manifest to the naked eye by the bending of the road. There must be plenty of good turf underneath it.

The transit hut had been erected last year, and a substantial brick pier built. The clock was soon set up in the "artificial cable" room, where the temperature is maintained nearly constant in the interests of this apparatus. A few words of explanation may be fitting.

For duplex signalling the cable is "balanced," both in resistance and capacity, by a composite conductor called an "artificial cable." The current from the Waterville battery is sent along a divided circuit, as in the figure; and if the arms A B and A C are properly adjusted, no current will cross B C, in which is placed the siphon recorder G. But signals from Canso of course affect the recorder. The adjustment of the balance, which must be very exact, would be upset by changes of temperature, and hence the care taken to keep the artificial cable in a room at uniform temperature, which we found so handy for the clocks at Waterville and Canso.

The hut was necessarily some little distance from the operating-room, but Mr. Wilmot soon ran some wires across, with bottle necks for insulators, and in a couple of days all was ready for stellar observation.

[The two days, by the way, were chiefly consumed in boring holes in hard stone with indifferent tools, which required constant resharpening.]



But it took a little longer to get the signals thoroughly in going order, partly because the astronomical requirements had to be explained to the telegraphists, and the telegraphic situation to the astronomers; and partly because both telegraphists and astronomers on opposite sides of the Atlantic had slightly different views. We found out ultimately that the best plan would have been to hand over the signalling entirely to the telegraphists, whose methods are, from commercial necessity, quite accurate enough for astronomical purposes. We had originally some fears as to the time of action of relays when signals were repeated through them; and possible variations of the time of transmission of the current when the strength was varied; and we caused Messrs. Wilmot and Dickenson and others some trouble by arranging an apparatus meant to guard against these variations, essentially the same as that devised by M. Loewy for use in the French survey. But it became obvious that we might have saved this trouble; for people whose daily occupation it is to send some few hundred words or many thousand signals a minute do not use apparatus likely to vary by many thousandths of a second.

While at Waterville, I exchanged signals with Greenwich at 8 and 11 o'clock, observing stars between exchanges; and with Canso at midnight approximately. For the signals with Canso the siphon recorder was used as a chronograph. The recorder is a beautiful piece of apparatus to see at work. The siphon is a short capillary glass tube, bent to the required shape by holding it near a match flame, and suspended on a fine silk fibre, with one end dipping in the ink reservoir, and the other touching the travelling paper tape, on which the record is made. It is agitated by a tuning-fork, so as to make (say) 40 dots on the paper every second. When no current passes across the bridge B C in the above figure, the row of dots is a straight line; but a very slight current sent across B C jerks the siphon sideways and scatters the dots into a series of waves, which gradually subside to quiescence. If a signal is received from Canso, the motion of the siphon is more gentle and there is a gradual swell in the trace. Slight jerks of the former kind were sent automatically across B C by the clock every two seconds; and the signals sent to Canso were also recorded as jerks of the trace by slightly upsetting the perfect balance with the artificial cable, so that every time a signal was sent in at A, a very small portion of it crossed B C and made a record. The signals received from Canso, which were in a similar manner recorded on the Canso tape, caused gradual swells in the Waterville trace, on which the sharp clock-beats could still be distinguished where necessary. Even in the case of these more gentle disturbances, there was found to be no difficulty in reading off the first deviation of the trace to something like $0^s.01$.

After the initial difficulties the work went well and quickly. Life was very pleasant in that beautiful corner of Ireland: there was even some primitive golf to be played—primitive in the sense that its chief exponent had learnt the game from books, and naturally misunderstood one or two important points, such as that a teeing ground and a putting green are not necessarily coincident; indeed, that their separation may be a positive advantage to the latter: but still it was excellent golf as regards the turf and the genuine enthusiasm of the said chief exponent and others. By no means least among the others were the descendants of Dan O'Connell, who live ten miles of the loveliest scenery from Waterville, and who promptly laid out an excellent links in their own domain after playing the game once at Waterville. I got Mr. Wilmot to hit a ball towards the end of my stay, and he now has a set of clubs. *Magna est Golfina et prævalebit!*

I was unfeignedly sorry to leave Waterville, in a closed carriage and pair. It rained cheerfully all the way to Killarney, and I don't know how many games of cribbage Mr. Wilmot played with me on the journey. During lunch on this occasion we were as well off as the horse, for we did not move from our comfortable seats under the carriage roof, and I think the cribbage still went on. One felt some anxiety about the telescope outside, but the event proved that it was well covered. H. H. TURNER.

[To be continued.]

Selenographical Notes.

MT. ARGÆUS AND ITS SURROUNDINGS.—The broad gap, some 70 miles in width, which breaks the continuity of the border of the Mare Serenitatis on the south-west is overlooked on either side by a noble headland rising steeply from a nearly level plain. That on the east,—the Prom. Acherusia, a cape-like extension of the Hæmus mountains and structurally associated with this range—attains (according to Neison) an altitude of nearly 5000 feet; the other, on the west, named Mt. Argæus by Webb, is a magnificent mountain mass towering sheer to a height of almost 9000 feet above the sea-level, and standing without a rival which can compare with it in size or height for miles around.

If viewed when the crest of the ring of Plinius is just coming into morning sunlight, a good idea may be formed of the characteristic features of this grand formation, which is at least 30 miles in length and trends from N.E. to S.W. The jagged shadows, which at this phase extend nearly up to the curious light plateau surrounding Dawes, show that its loftiest peak is at the extreme north-east and that it gradually decreases in altitude towards the south, ultimately sloping down somewhat suddenly to the dark-coloured plain, where its extremity is marked by a shallow circular ring filled with black shadow. The shadows reveal besides the existence of a narrow deep ravine or pass which traverses the mountain at a point about one-third of its length, reckoning from its northern end. Near this is a short rocky projection on either side, which gives the whole the appearance of a spear-head or javelin, the point lying a little west of the small ring-plain just referred to.

Though no neighbouring objects can vie with Mt. Argæus either as regards bulk or altitude, the dusky plain on its N.W. flank, which, according to Beer and Mädler, is nearly 2500 feet above the level of the Mare on the opposite side of the mountain, includes three bright isolated hills of considerable height, together with others situated more towards the south-west, pertaining to the broken surface north of Vitruvius. Amid these, a little west of the southern end of Mt. Argæus, stands a brilliant little crater, which may be easily missed at sunrise, though it is very conspicuous under a high light. Mädler shows only a little mound on its site and Neison has slightly misplaced it. The great Littrow cleft, discovered by Schmidt in 1860, runs from near Dawes close under the towering northern cliff of Mt. Argæus. A very favourable view of this was obtained at 8^h on April 4, 1881, with a power of 200 on a 4-in. Cooke achromatic, Plinius being just within the morning terminator. It is by no means an easy object and has often been sought for since without success in a larger instrument, used under apparently equally favourable conditions.

Most observers are familiar with the contrast in surface-colour between the two adjoining great seas, the Mare Serenitatis and Tranquilitatis, and with the strikingly abrupt transition from the variegated greyish tone of the former to the much more sombre hue of the latter. It is evident enough on all good photographs. The line of demarcation lies almost evenly between the Prom. Acherusia and the northern end of the Argæus mountain, and appears to consist of a dark band extending across the gap. The northern boundary of this dark strip is in part, at all events, the site of the rill marked ε in Neison's map, which, originating near the northern side of the promontory, runs through the region north of the Dawes plateau towards Mt. Argæus and the Littrow cleft, which it probably joins. There has been some uncertainty as to whether this object originates on the south or north flank of the

Hæmus mountains. Schmidt represents it as beginning on the north, and on several occasions I have traced it very distinctly from a point some distance north of this range, following the curvature of the coast-line of the M. Serenitatis. It is by no means an easy object, but it has been seen very satisfactorily with a 4-in. achromatic, and its course (convex towards the south) followed through the great Serpentine ridge and other minor elevations which it traverses, like a railway, cutting through ranges of hills. In what way, if at all, the cleft, which divides so sharply the colour-contrast between the two great "seas" is related to this sudden change of surface tint will probably never be determined, though the connection is clearly too distinct to be merely the effect of chance. It may be that a general sinking of the central area of the Mare Serenitatis occasioned the linear crack in this particular part of its periphery, and thus a welling out of liquid matter which, flowing northwards, down the slope of the depressed surface, formed the narrow band of dark colour which we see, though this affords no explanation of the dusky hue of the Mare Tranquilitatis, of which this cleft and band are the northern limits. It has, indeed, been urged that such a sinking accounts for the clefts which, it is said, roughly follow the western shore-line of the Mare Serenitatis. But it seems to me that this hypothesis fails, because, as a matter of fact, the great Littrow chasm, and other clefts nearly as important, run for the greater part of their course through the mountainous region at the *back* (or west) of the bordering formations, Le Monnier, &c., and, except, for a comparatively short distance, not on the surface of the Mare at all.

As most lunar observers know, two other clefts, θ and δ, originate amid the Hæmus mountains at no great distance from ϵ, but on the southern side of the range,—all three radiating in W.N.W. direction from this region. Of these, the more southerly, δ, is in part a crater-rill and is by far the most conspicuous. The middle one, θ, is also not difficult. Both are sometimes easily traceable when the morning terminator has advanced as far as west long. 10°. On January 28, 1882, 5^h to 6^h, I noted two parallel clefts, about midway between θ and δ, on the light plateau south of Dawes, the more southerly and longer of the two terminating on the west at a minute craterlet, a second small craterlet standing about midway between its extremities. The eastern ends of both these clefts abut on a ridge running north and south. On many occasions an object resembling a short cleft was traced from one of the crater-like expansions of δ, at a point nearly due south of the centre of Plinius, to the rill θ. It is not shown on any of the maps and may possibly only be one of the long narrow spurs extending from the outer slope of the neighbouring ring-plain, though with a power of 340 on my 8½-in. Calver it had all the appearance of a true cleft.

The light-coloured area round Dawes is another noteworthy feature in this interesting region. Whether it really is raised or not above the level of the Mare, is hard to say. Under some conditions it resembles a plateau standing considerably above the surrounding plain. But a low Sun reveals the existence of a ridge of some height encircling it. With the exception of the rills just mentioned and a curious depression a little to the east of them, well seen on April 2, this year, its surface appears to be wholly devoid of detail. THOS. GWYN ELGER.

Beaumont House, Shakespeare Road,
Bedford, 1892, Oct. 19.

CORRESPONDENCE.

To the Editors of 'The Observatory.'

Jupiter.

GENTLEMEN,—

As there are considerable changes in the aspect of the disk-markings on Jupiter since August, I venture to send you appearances up to date as seen in moderate apertures. The following items are based on drawings and reports which have come to hand in connection with the sectional work of the British Astronomical Association. There is the more need that special attention should be paid to these markings, for many of them are small dark spots, and may readily be mistaken, even by experts, for the shadow of the fifth satellite, or possibly for that of asteroids passing the disk of the planet in the plane of vision. The intense interest excited among Jovian students by Mr. Barnard's reported discovery may well account for the earnest search for the satellite, or its shadow, even among those whose apertures are theoretically inadequate.

1st. The Red Spot is believed by some observers to be more manifest than in the earlier days of the present apparition. Mr. MacEwen, of Glasgow, quotes it as of "a beautiful pale orange colour, and quite easy in his 5-in. O.G. on the 3rd of Oct." At the end of August, it "was quite invisible to him." While greater visibility may be attributed in part to greater altitude during amateur's observing hours, the collective evidence goes to show we shall not altogether lose that most interesting feature of the Jovian markings during the current apparition.

2nd. The South Polar region is becoming much more disturbed. Mr. Offord in a sketch shows a distinct marking around the pole itself, a very unusual circumstance, owing to general loss of definiteness, from unfavourable perspective position.

3rd. The dark streak, so marked a feature of the apparition of 1891, has again become visible on the South Temperate band. Though comparatively faint, it seems to be increasing in intensity, Its duplicity in longitude is suspected. On the south-east of this marking there is another similar one, much fainter, and, as the present writer conjectures, a thickening of the band south of the S. temp. one, which has been seen in his 12½ speculum extending from limb to limb. These markings demand careful observation on the part of Jovian students. Mr. A. Stanley

Williams has furnished ephemerides of the above to assist in determining the rotation-period, which is much swifter than that of more permanent markings.

4th. The south equatorial belt is wider and more disturbed than last season. It is also lighter in tint, being of a very delicate reddish brown. There are several dark markings on it, especially on its northern edge. There is a curious rift extending right across the belt, an extension, in fact, of a white spot to which Mr. Williams has called special attention, and furnished an ephemeris of its passage across the central meridian.

5th. On the north temperate belt there are several dark spots closely resembling satelline shadows. They are visible on the north and south edges and on the body of the belt. Ephemerides, as constructed by Mr. Williams, are issued to intending observers. The colour of this belt is very intense and of a copperish tint.

6th. The North Temperate band is also becoming more intense, taking rank in prominence with the North Tropical one; there are more evidences of coming activity than two months since.

7th. The bluish tint of the north polar region is very apparent this apparition, and is quite believed to be wholly objective in character, suggesting the possibility that that region is cooler than the tropical ones.

The above particulars are additional, and partly corrective to the notes dated Aug. in your last issue, data having considerably accumulated since then. Yours faithfully,

Portland, 1892, Oct. 10. W. R. Waugh.

Minima of R Ursæ Majoris.

Gentlemen,—

In the 'Companion to the Observatory' for 1892, this star bears the No. 63 in the list of Variable Stars on page 23. Referring to the table of Maxima and Minima on page 30, I find the minima for this star are given on August 23rd and September 22nd. I should therefore be glad of any information as to which of these dates is correct.

R Ursæ Majoris has of late been very faint. The magnitude on June 11th was 11·1, on August 24th it was 12·2 magnitude, and when last observed on October 5th the magnitude was 11·9.

Rousdon Observatory, Lyme Regis, Cuth. E. Peek.
Dorset, 1892, Oct. 14.

[Aug. 23 is the correct date. On Sept. 22 there is a min. of R Ursæ Minoris, No. 116 in the list; hence the mistake, which we regret.—Eds.]

OBSERVATORIES.

Liverpool.—The new director, Mr. W. E. Plummer, writes to us that he finds the Library very ill supplied with books, and that this is more particularly the case with star catalogues and books of reference. The work of the Observatory is much hindered by this want, and we would suggest to any of our readers, who have duplicate copies of any astronomical works with which they are willing to part, that they should offer them to the Liverpool Observatory, where they may be sure they would be valued, and, what is of more importance, used.

MADRAS.—Mr. C. Michie Smith has been officiating astronomer since 1891, July 11, and presented the annual report on 1892, May 23. We believe no definite successor has yet been appointed to Mr. Pogson, but Mr. Smith is pushing forward the printing of observations with considerable success. His report opens: "The present year being the centenary of the founding of the Madras Observatory, a brief historical sketch may not be thought out of place in the present report." The original equipment was transferred as a gift from his private observatory by William Petrie. The first astronomer was Goldingham, 1793–1829, whose observations are apparently still in MS. at Madras. Taylor (1830–1848) made the famous star catalogue bearing his name. This is now nearly out of print, but is still applied for. It would be well if some competent person could undertake the revision of this catalogue with a view to another edition. The MSS. containing the original observations are at Greenwich, and from a casual inspection I believe an editor would do valuable work. To undertake such a task at Greenwich with the present staff is of course hopeless. Mr. Michie Smith suggests a reprint; but this would possibly diminish the chances of an ultimate revise, and for this reason is to be deprecated. Captain Jacob was astronomer from 1849–1859, and instituted equatorial observations; and after two years Mr. Pogson was appointed in 1861, and held office until his death a few months ago. A new and good instrumental equipment (transit circle and 8-inch equatorial) was employed to good purpose by Mr. Pogson during his tenure of office; but there has been the old astronomical difficulty about publication. It is not for us to judge whether this was Mr. Pogson's fault or his misfortune; the important fact is that the publication is now going forward energetically, "observations being held as altogether subordinate, except as regards those necessary for the time service." Under these conditions it is perhaps hardly the time to consider the erection of new instruments, unless a large addition is to be simultaneously made to the staff.

H. H. T.

MELBOURNE.—We are sorry to learn that the grants of money to this Observatory are to be reduced. Messrs. White and Moerlin, the 1st and 2nd assistants, retired on pension on Sept. 30; and Mr. Ellery, the Director, is to remain a few years longer at the helm with a view to reorganizing the staff on a reduced scale; all salaries being incidentally reduced 7½ per cent. The news is not cheerful!

Abastuman.—A new mountain observatory has been established at Abastuman ($2^h\ 51^m\ 25^s$ E., $+41^\circ\ 42'\!\cdot\!4$). It is 4500 feet above sea-level, and is equipped with a 9-inch telescope by Repsold.

PUBLICATIONS.

Jupiter and his System. By **Ellen M. Clerke.** (Forty-four pages in a paper cover, price 1*s*.)—One is inclined to rub one's eyes at sight of the first name of the authoress, and again after glancing through the work and recognizing a familiar style. But we are very glad to be able to congratulate the said authoress on her well-conceived new departure in the matter of the "shilling shocker." The shocks are of course the big numbers which represent astronomical facts, for there is no sensational writing; and glad will many be to find that so able and sober a pen is the first to introduce astronomy into railway literature. There are one or two shocks of another kind for some of us; as, for instance, when we read on page 26, "The navigator having the dates (of the eclipses of Jupiter's satellites) tabulated, can, by observing the actual moment of their occurrence, correct his chronometer with as much confidence as though he were within hearing of gun-fire at Greenwich." We hope that before the second edition the authoress will have an opportunity of observing an eclipse of a satellite; and, by the way, there is no gun-fire at Greenwich. But such slips are few indeed; and there is so much patiently collected and condensed information in the few pages that these can be readily overlooked. That the information is up to date will be recognized from the fact that the fifth satellite occurs not only in the text but on the cover. He is perhaps a little definite; and the others rather too much alike in size; but this must be put down to exigencies of cover-illustration.

Researches in Stellar Parallax.—Part IV. of the publications of the Oxford University Observatory is another instance of a complete and most valuable investigation carried out by Prof. Pritchard and his assistants. The work actually due to Oxford is a determination of the parallaxes of 30 northern stars of the second magnitude; but a complete catalogue of known parallaxes is added, and in an introduction the history of the whole subject is carefully given.

At Oxford the photographic method previously employed for 61 Cygni and other stars was used in this more general investigation. Small plates were exposed in the focal plane of a reflecting telescope "at the critical epochs of the parallactic motions of the various stars discussed." Prof. Pritchard has concluded "from actual and prolonged experience that an accuracy, amply sufficient in the present condition of astronomy, is secured by observations of each star made on 25 nights advantageously selected throughout the parallactic year, four exposures being usually made on each night."

The general result of the investigations is that the average parallax of a star of the second magnitude is 0″·056; and comparing with this the result of Drs. Gill and Elkin for the average parallax of 14 first-magnitude stars, viz. 0″·089, we see that there is distinct evidence that the brighter stars are nearer. The employment of the photographic method in the former case, and the heliometric in the other, must not be lost sight of as a possible source of systematic difference; but "throughout each series the parallaxes have been determined by the same mathematical treatment and generically by the same method."

Although those interested in the subject should at once obtain the memoir, which is invaluable as a work of reference, the following condensed table of results will doubtless be of immediate interest to those at a distance. The two determinations for each star are due to different comparison stars.

Star's Name.	Parallax.	
	a.	*b.*
	″	″
α Andromedæ	+0·0565	+0·0600
β Andromedæ	+0·0610	+0·0860
α Arietis	+0·0880	+0·0715
α Persei	+0·0996	+0·0738
β Persei	+0·0642	+0·0529
β Tauri	+0·0730	+0·0529
β Aurigæ	+0·0591	+0·0652
γ Geminorum	−0·0135	−0·0333
α Ursæ Majoris	+0·0486	+0·0436
β Ursæ Majoris	+0·1177	+0·0434
γ Ursæ Majoris	+0·0768	+0·1206
ε Ursæ Majoris	+0·0832	+0·0792
η Ursæ Majoris	−0·0309	−0·0628
β Leonis	+0·0490	+0·0087
β Ursæ Minoris	−0·0200	+0·0644
α Coronæ	−0·0255	−0·0493
γ Draconis	+0·0625	+0·0371
γ Cygni	+0·1107	+0·0931
ε Cygni	+0·0927	+0·1629
α Pegasi	+0·0913	+0·0719
ε Pegasi	+0·0693	+0·0919

The probable error of each of the above results is +0″·025 within a very small quantity.

Now that Mr. Plummer has left the Observatory, it may be a fitting moment to recall that since the Observatory was founded in 1875 by Prof. Pritchard's energy, aided by the generosity of the late Dr. De la Rue, no less than four complete investigations, not to mention other excellent work, have been carried out by the Director and the same two Assistants, viz.:—A Triangulation of the Pleiades, An Investigation of the Moon's Libration, the 'Uranometria Nova Oxoniensis,' and the present determination of parallax.

CATALOGUE OF THE 'ASTRONOMISCHE GESELLSCHAFT,' Vth Part.—The section of this work allotted to the Harvard College Observatory, namely, the observation and reduction of the stars in zone +50° to +55° North Declination, is now completed, and the catalogue published. In the formation of this catalogue the system of selection of fundamental stars, as planned by the Gesellschaft, was not precisely followed, for the requirements of the United States Coast Survey made it necessary that stars outside the allotted zone should be used as fundamental. A discussion of the stability of the instrumental constants has been made and published in vol. xvi. of the 'Annals' of the Observatory, to show that this modification of the plan does not impair the value of this section.

The observations were made in the years 1870–78 and 1883–84, principally by Prof. W. A. Rogers, under whose direction the catalogue has been published; the Right Ascensions were observed chronographically over eleven vertical threads, and the declinations also chronographically over an inclined thread, the circle being read by two microscopes. An exhaustive discussion of probable error in the Introduction to the Catalogue shows that (1) the P. E. of an observation in the years 1870–78 is in R. A. $\pm 0^{s}{\cdot}054$ and in Declin. $\pm 0''{\cdot}55$; (2) that the P. E. is rather larger for stars fainter than magnitude 8·0 than for brighter stars, and that the observations in 1883–84 are inferior to those in the earlier years, which fact Prof. Rogers ascribes to the want of an assistant as recorder, and consequent physical difficulties under which the observations were made. H. P. H.

THE PLANET MARS*.—"What is worth doing at all, is worth doing well," and M. Flammarion has thought that a monograph upon this most interesting planet was worth doing, and has carried out the proverb to the letter. A more painstaking and complete treatise could scarcely be imagined. In a volume of more than 600 quarto pages, and illustrated by nearly 600 drawings of the planet and by 23 charts, M. Flammarion gives a complete history of the observation of Mars from the earliest days of the telescope to the present opposition. The delineation of the surface, the changes observed in it, the condition of the planet—its seasons, its shape, dimensions, orbit, satellites, and habitability,—are all treated in turn with conspicuous thoroughness, and the result is a volume which, for the completeness of its information, one could hardly even imagine to be surpassed. To the amateur astronomer to whom this planet has so often proved such a fascinating object, M. Flammarion's work will be especially valuable; and to every one who seeks for information on the subject it will prove a complete record of all that has been achieved up to the present time.

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* 'La Planète Mars et ses Conditions d'habitabilité. Synthèse générale de toutes les observations.' Illustré de 580 dessins télescopiques et 23 cartes. Par Camille Flammarion. Paris: Gauthier-Villars et Fils, 1892.

NOTES.

Thermal Absorption in the Solar Atmosphere.—Mr. E. B. Frost has made (at Potsdam) a series of observations on the Sun's disk with thermopiles of his own construction. His results confirm those of Langley and others in showing a diminution of radiation towards the limb, owing to absorption by the solar atmosphere, intermediate in amount between that for red and yellow rays. There are indications that spots radiate *more* freely than the adjacent photosphere. Mr. Frost spells litre, liter; it is probable that the French will not be so anxious to have the metric system universally adopted if this is the way it is going to be spelt. (A. N. 3105.)

Victoria Comparison Stars.—"The relative positions of the comparison stars employed in the observations of Victoria in 1889" (says Dr. Gill in A. N. 3107) "are now, thanks to the generous and hearty co-operation of a large number of astronomers, more accurately known than those of any other group of stars in the heavens, and may therefore be advantageously used for determining the optical distortion and scale-value of our photographic telescopes, and testing the various methods for the 'raccordement des plaqués.'" The places depend on 3766 meridian observations of R.A., and 3771 of Declination, combined with 1867 heliometric observations of distance and 151 of position-angle. The combination is performed by Dr. Gill in a masterly manner in the paper quoted. The stars extend from $19^h\ 21^m$ to $20^h\ 5^m$, and are between -3° and -9° declination.

Orbital Motion of Procyon.—Dr. L. Struve finds from micrometric comparisons of Procyon with two 9th magnitude stars that the radius of the orbit of Procyon determined by Dr. Auwers in 1873 is too large. Instead of $0''\cdot98$ it should be $0''\cdot74$. But a circular orbit still satisfies the observations well, and there seems no reason to compute elliptic elements. (A. N. 3108.)

Discovery and Announcement.—In friendly controversy with Prof. Barnard as to the prior right to a nebula which Prof. Barnard had discovered a year before Mr. Denning, but never announced, the latter suggests that "it would be an excellent plan if the R. A. S. or some other competent authority published each year a summary of the astronomical discoveries. This is already done as regards minor planets, but the list ought to include new nebulæ, double and variable stars, &c. . . No doubt the additional information might be easily collected from observers and formed into pretty complete tables."

Comet Notes.—Brooks' Comet is now quite a conspicuous object even in a small telescope; on Oct. 23 it was noted as large and round with decided central condensation. No ephemeris is to hand beyond Oct. 31, but the following are approximate positions for the beginning of November:—

	R.A.			N. Decl.			R.A.			Decl.		
	h	m	s	°	'		h	m	s	°	'	
Nov. 4....	9	27	51	9	10	Nov. 12..	10	1	13	2	21	N.
8....	9	44	12	5	55	16..	10	18	58	1	20	S.

Both Swift's and Denning's Comets are now beyond the reach of ordinary instruments, but an ephemeris may still be useful for the possessors of large instruments.

Denning's Comet.

	R.A.			S. Decl.	
	h	m	s	°	
Nov. 5	5	52	56	0	58
9	5	46	55	2	32
13	5	40	27	4	16

Swift's Comet.

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	'		h	m	s	°	'
Nov. 5....	23	44	8	37	14	Nov. 17 ..	23	45	3	34	7
11....		44	11	35	37	23 ..		46	40	32	44

A very faint comet was discovered photographically by Barnard on Oct. 12.

Its approximate place on Nov. 3rd is R.A. 20^h 20^m, N. Decl. 4°, daily motion 4^m East, 25′ South.

Minor Planets.—Since the last note on this subject (in the May number) there have been several new planets discovered, all by photography. In consequence of the confusion that was arising in the numbering it has been decided to provisionally distinguish new discoveries by letters and to postpone assigning permanent numbers till the close of the year. It seems, however, unfortunate that the letter A has not been assigned to the first discovery of the current year. The following is the list of discoveries:—

A Aug. 22 (Wolf) ; B Sept. 1 (Wolf);
C Sept. 1 (Wolf) ; D Sept. 19 (Charlois);
E Sept. 22 (Charlois); F Sept. 25 (Charlois);
G Sept. 25 (Wolf) ; H Sept. 25 (Wolf);
J Sept. 25 (Wolf) ; K Oct. 20 (Wolf).

B in the above list proves to be identical with (163) Erigone.

The plate on which G and H were recorded also showed (34) Circe and (184) Deiopeia.

Planet 308 has been named Polyxo; 322 Phæo; those discovered by Wolf on Jan. 20, March 18, March 28 have been named respectively Brucia, Heidelberga, and Ilmatar; and that discovered by Charlois on March 22 Columbia. 311 and 312 have just been named Claudia and Pierretta.

New Catalogue of Variables.—Mr. Chandler is about to prepare a new edition of the valuable Catalogue published in the 'Astronomical Journal,' Nos. 179–80 and 216. All observations not hitherto published should therefore be sent to him as soon as possible to help in the good work.

British Astronomical Association.—The Editors regret that, through some mistake, blank sheets were sent them instead of the account of the Meeting on October 26, and there was not sufficient time to rectify the mistake before going to press. The account of the Meeting will be given next month.

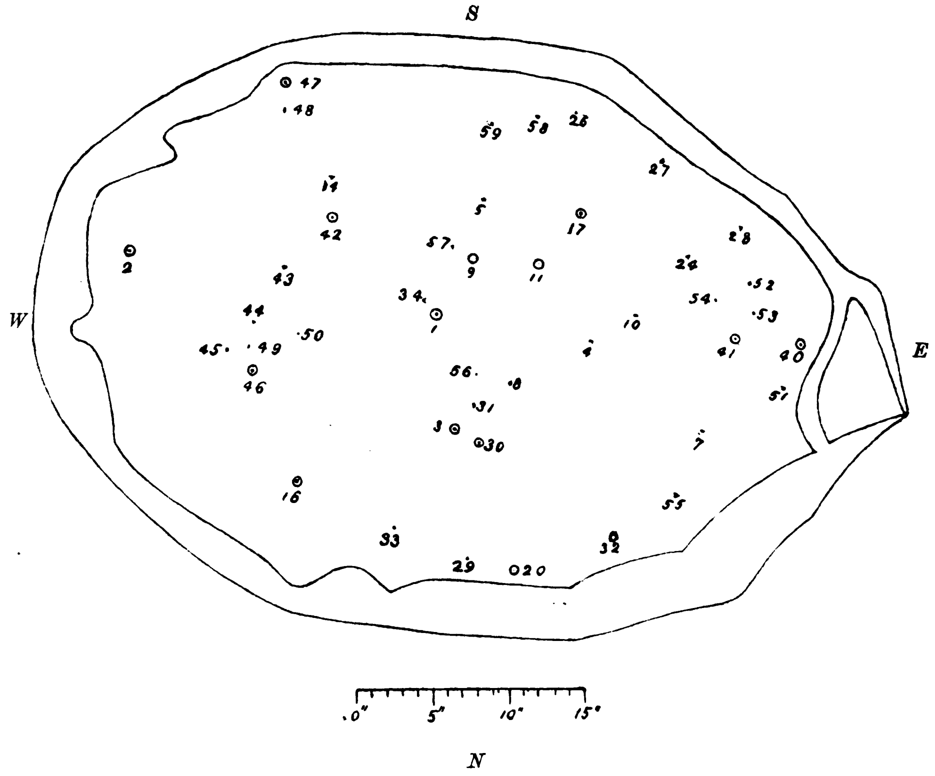
A collection of astronomical printer's errors would be amusing reading. Here are two of recent occurrence:—

"Professor Porter, of the Cincinnati Observatory, stated to-day that Dr. Jahrsbuch, the Berlin Astronomer, gives the time of the opposition of Mars to the Sun as just after midnight on August 6."—*Standard*, August 5.

The personification of the 'Berliner Jahrbuch' is doubtless due to the expansion of a telegraphic message, as also is the following instance. A representative of the 'Central News' enquired at Greenwich about the proposed large telescope for the Paris Observatory, and heard incidentally of the Lick 3-foot refractor and Mr. Common's 5-foot reflector. In a Welsh paper next morning was a paragraph referring to "the lick 3-foot refractor or more common 5-foot reflector!"

We congratulate Mr. A. A. Rambaut on his election to the Chair vacated by Sir Robert Ball. Mr. Rambaut has been ten years at Dunsink, and hence will be quite at home in his new office.

PLATO.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THE OBSERVATORY,

A MONTHLY REVIEW OF ASTRONOMY.

No. 195. DECEMBER. 1892.

MEETING OF THE ROYAL ASTRONOMICAL SOCIETY.

Friday, November 11, 1892.

E. B. KNOBEL, *President*, in the Chair.

Secretaries: H. H. TURNER, M.A., B.Sc., and E. W. MAUNDER.

THE Minutes of the previous Meeting were read and confirmed.

Mr. Maunder. 209 presents have been received since the date of the last Meeting. That number does not include, however, one particular present, which is displayed on the mantelpiece, namely photographs of the Past Presidents of the Society, presented by Mr. Schooling, which he intends to be part of a complete series of portraits of Presidents of this Society. Among the other presents that are worthy of special notice is a photograph of the Moon from the Lick Observatory, and a number of photographs received from Mr. Russell, of the Sydney Observatory, who has sent a paper concerning them. Amongst the books received is a work by M. Flammarion on the Planet Mars, a very exhaustive monograph of 600 pages, and 600 drawings of the Planet from the earliest stages to the present time, and some 23 or 24 maps of it. Of the other books we have the first publications of the Vatican Observatory, Vols. i. and ii.; and the 2nd Vol. of the publications of Von Kuffner's Observatory in Vienna, and a catalogue of 3950 stars by M. G. Bossert ('Supplément à l'Histoire Céleste de Lalande').

The President. In asking you to return your thanks to the donors of these presents I cannot help referring to the very handsome present Mr. Schooling has made, and is making, to the Society in presenting to us a series of photographs which in a measure will represent the history of the Society. [Hear, hear!] The Presidents of this Society from 1820 number among them those who in the past have been most distinguished in the annals of English astronomy; therefore their reproduction deserves a special vote of thanks from us to Mr. Schooling for his gift to the

Society. [Hear, hear!] I therefore ask you to return your thanks to the donors, especially to Mr. Schooling.

The proposition was carried by acclamation.

Mr. Turner then read a paper by *Mr. Barnard*, entitled "Note on the period and distance of the 5th Satellite of Jupiter." This satellite was discovered by the writer at midnight, Sept. 9, 1892, with the 36-in. equatorial. From the eastern elongations of Sept. 10 and Oct. 21, the following periodic time results: $11^h\ 57^m\ 20^s\!\cdot\!5$. Hourly motion $30^\circ\!\cdot\!111$. The period of $17^h\ 36^m$ distributed in Europe was an error in telegraphic transmission. The period of $11^h\ 50^m$ sent later was erroneously derived from the observed distances of the planet's satellite. From seven eastern observed elongations the distance of the satellite (at East elongation) is $48''\!\cdot\!094$ (at $5^h\ 20^m$). This corresponds to a distance from Jupiter's centre of 112,510 miles. The few observed western elongations give a distance about $1''$ less than this, and there is evidently sensible ellipticity to the orbit. At present the satellite is assumed to be of the 13th magnitude. Under the most favourable conditions it has not been possible to see the shadow of this object when in transit. From these and other considerations the satellite is probably not over 100 miles in diameter. Taking everything into consideration, the brightness at east and west elongations is the same. Following are two of the last observed *east* elongations of the satellite:—1892, Oct. 21 $17^h\ 2^m\!\cdot\!8$ G.M.T., computed from measures before and after elongation; 1892, Oct. 23 $16^h\ 49^m\!\cdot\!0$ G.M.T. (measured distance $62''\!\cdot\!90$). As there has been considerable doubt as to the various announcements as to the discovery of this satellite, which is a matter of extreme interest, I think I shall not be wrong in reading a letter from Prof. Holden which he has addressed to the Editors of 'The Observatory' (see page 452).

Mr. Freeman then read a paper on the same subject. He was pleased to see that Mr. Barnard had discovered his mistake as to the $11^h\ 50^m$, which he admitted to be entirely erroneous. He (Mr. Freeman) had taken the measures that were published in the 'Astronomical Journal,' No. 275, and reduced the apparent elongations to what they would simply be if the satellite of Jupiter had been viewed from the Sun at the mean distance of Jupiter from the Sun. He found three results, and the mean of the three was $48''\!\cdot\!05$ practically, and Mr. Barnard gave $48''\!\cdot\!094$. He made the period $11^h\ 58^m\ 49^s$, but he remarked that it was derived from too few measures of distance. The true period could only be derived as it had been found by Mr. Marth.

The President. I would invite remarks on this remarkable discovery of Mr. Barnard. It is a long time since astronomers have received such an announcement as that which came before us a few weeks back as to the finding of another satellite of Jupiter. The observations accord so well that we can have no doubt whatever as to the reality of the object; and the importance of the discovery is very much enhanced by the remarks which have since been made about it.

Mr. Turner. I fancy now that we have the true period of the satellite before us, and there is every reason why it has not yet been seen in England, viz. because the satellite was behind the planet when Dr. Common looked for it. The period is apparently about 12 hours, so that elongations occur nearly at the same hour for many nights consecutively; and it is quite possible that Dr. Common, although he looked on several nights from 9 to 1, was looking for the satellite when it was behind the planet. There may be many reasons why no one else has seen it; but now that astronomers have new light on the question they may begin looking for it again under more favourable circumstances. Big telescopes are not very numerous; and as regards Europe observers may have been looking at the wrong time: at Washington they are moving the observatory, and the big telescope is not readily available; and so on. It would not be difficult to give reasons why the satellite has not yet been seen elsewhere than at Lick. I firmly believe it is a discovery of great importance, and one may hope it will in the near future be confirmed in the completest manner.

Mr. Maunder then read a paper by *Mr. Burnham* on "The motion of Zeta Cancri." The writer said that in a paper on invisible double stars which appeared in the 'Monthly Notices,' 1891, he gave a notice of this star. The question to be solved was whether the apparent want of uniformity in the motion of stars was to be ascribed to the disturbance of some unknown invisible body or to the errors of observation. It was obvious that this should be adopted as the explanation of actual observation only when the ordinary means of accounting for discrepancies had been shown to be insufficient. There was a possibility that explanation might be found in the errors of observation, otherwise they were forced to admit the existence of many more dark suns than one would expect to find in a single class of binary stars. All the measures, Mr. Burnham wrote, were made near the meridian and under favourable atmospheric conditions, and he relied principally upon the measures of declination, but it would be well to continue all the measures of the stars in use.

Mr. Ranyard. This is a very interesting communication. With regard to Mr. Burnham's remark that there is no evidence proving the existence of dark stars, of course the Algol stars afford evidence that there are dark stars. We may feel certain that the companion which produces the occultation is not bright compared with the principal star, or a secondary minimum would be observed when the star, which produces the eclipse, passes behind the bright star.

Mr. Turner then read a paper by *Mr. Barnard* on the "Discovery of a Comet by Photography." A comet was discovered by this means on Oct. 12 in the Milky Way. On developing the plate used on the night of the 12th, he observed a distinct hazy trail; and on the following night, as soon as it was dark, another search was made with the 12-inch, and the object was found nearly a degree south, and observations showed it to be a comet with motion S.E.

Mr. Holmes (being called upon by the President) said: I have been in the habit of looking at the great nebulæ in Andromeda on every possible occasion. On Sunday night last things were not very favourable, but I thought I would look through the telescope once more. On turning to the finder I saw something hazy, and on going to the eyepiece I found there was something fresh to me. I very quickly recognized that the object was a new comet. The nucleus was quite bright. Before I could get its exact position clouds came up. I, however, sent the rough position to Mr. Maunder and other friends. The next morning I wrote to the Astronomer Royal and to Mr. Kidd. Mr. Kidd at once wrote to say it was suspiciously near the nebula, but I said I had not made a mistake like that. On Tuesday morning I got a postcard from him saying that he and two other gentlemen had seen it at 6 o'clock with the naked eye. In one way the discovery may be regarded as an accident; but if I had not been intimately acquainted with that part of the sky I might not have seen it. I have not been able to get another observation since. Wednesday night was very thick; I got a glimpse of the comet, but I could not obtain an observation to tell you how fast it moved or to identify any stars near it. It has been suggested that the elements are very much like those of Biela's comet, but I believe that broke up entirely, and I never heard of one gathering itself together again; besides, I fancy this is a good deal bigger than Biela's comet. I am pleased the discovery has been made in this country and also that it has fallen to an amateur, as it may encourage others to search.

The President. I am sure we shall all desire to congratulate Mr. Holmes on this discovery. I apprehend Mr. Barnard's is the second instance of a comet being discovered by photography. Were there any more?

Mr. Ranyard. Yes; there are two solar eclipse ones—one of 1870 and the other the Egyptian eclipse.

The Astronomer Royal read a paper on the "Photographic Magnitude of Nova Aurigæ." He said I have felt a little doubt as to whether it was fair to overload the 'Monthly Notices' with the tabular matter which this paper involves, but I thought as this is a new subject it might be interesting to Fellows to have some details given of the determinations of the photographic magnitude which have been made of Nova Aurigæ from the time of its first appearance up to the present time, and also the variations of what is called the constant of the photographic plates and the magnitudes of the comparison stars. The paper consists in a large measure of tabular matter, but one or two points of interest may be mentioned. In the 'Monthly Notices' for last March there was a paper giving the magnitudes of the Nova up to March 9. Those were determined from measures up to that date, and those measures have been since supplemented by measures of other photographs up to April 1, when the star had fallen below 14 mag. photographically, and afterwards by measures of other photographs taken on Aug. 30 and Sept. 3, 5, and 6, when the star had apparently brightened again and was about 12 mag photographically. The measures have been discussed in the same way as in the previous paper, and I have formed a table of the magnitudes of the Nova for each day of observation at Greenwich. In regard to these it must be distinctly understood that they are photographic magnitudes, and it does not follow that they should correspond even generally with the visual magnitudes. By April 1 the Nova appears, according to our photographs, to have fallen below 14 magnitude. On March 30 the Nova was shown with an exposures of 10 minutes and on April 1 with 30 minutes. When it reappeared on Sept. 6 it was shown with an exposure of 1 minute. That will indicate the variations of brightness and the brightening which has occurred on its reappearance. The star has not been photographed since that time, as it came at an awkward time in the morning. I have given a table of the values of the constant and of the magnitudes of the four comparison stars, with a view to show the degree of accuracy which may be expected in determinations where we have stars of about the same magnitude to compare with, and it is very remarkable how consistent these measures are. [Exhibited two photographs on screen taken, one on March 30 and the other on Sept. 3.] One point arose about the question of the brightening of the Nova after its reappearance at the end of August, and that was that several observers described it as nebulous; and certainly there was great difficulty in measuring the diameter of the photographic image. Different observers differ as to the appearance of the image, which, so far as we can judge, is intrinsic in the star itself. [Exhibited on screen two photographs of the same field of stars—one for the Photographic Chart with a single exposure of 40 minutes; the other with three exposures of 20 minutes each, the images forming a triangle 5″ in the side, as proposed by MM. Henry.] It will be seen that we do not gain anything by having three images, because we get a blur, which might be mistaken for a stain on the plate.

The President said that there was a communication from Mr. Stone on observations of Nova Aurigæ.

Mr. Stone. I do not think they require any special remarks except that observers have made up their minds that the disk is not like an ordinary star.

The President. More like a planetary nebula.

Mr. Stone. I do not think they go so far as that, but they have made up their minds that the star has not got quite the appearance of the other stars around it, and it is very difficult to estimate the magnitude. They do not go so far as to say it is in the nature of a nebula.

Mr. Freeman. I have seen this Nova Aurigæ several times, and it seems always to be nebulous. We cannot get an image of it. All the stars appear to be apparently sharp, but the Nova is not very sharp. It always has a nebulous appearance, and you cannot always be sure whether that small nebulous mass has a real nucleus or not.

Mr. Maunder read a paper by *Mr. E. E. Barnard* on "Nova Aurigæ," and said the gist of the paper was the nebulous appearance of the star. It was first seen in August last, and in observations made since then the nebulosity had been a striking feature. At the last observation, made in October, the bluish-white colour was distinctly marked. The observations were made with the 36-inch.

Mr. Maunder read another paper on the new star in Nova Aurigæ by *Mr. Burnham* concerning its magnitude during the months of March, April, and May. Observations showed that the star retrograded very steadily in magnitude from 8 to 16. The nebulous character of the object was to be seen at once by an experienced observer.

The President. I should like to ask Father Sidgreaves whether he has noticed any change in the spectrum.

Father Sidgreaves. That is quite out of my reach; I have only an 8½-in. instrument.

Mr. Turner. When at the Harvard Observatory lately I found Mrs. Fleming had some photographs of the spectrum of the Nova, and there were two bright lines in the spectrum, and on two other days two other bright lines appeared. We were talking about many matters, and I forgot to get the actual details; I hope Prof. Pickering will soon publish the details.

Father Sidgreaves said he had brought with him a few slides to offer to the Members as a sort of interlude to the more scientific things brought forward and as a relief to the audience. They were plates of the solar spectrum in the region of the green and yellow, a little short of D, and they were taken on Edwards' isochromatic plates. Much to his astonishment, he found these plates were sensitive so far as C. He had come to the conclusion that he had no evidence that there was any real widening. Many amateurs had said that it appeared to them that the line had widened, but none of them were sure. He was known to be sceptical as to the widening of the lines, but his eyes were not what they were twenty or thirty years ago. Father Sidgreaves exhibited various slides by means of the lantern.

Capt. Abney. I have made a good many photos of the sun and sun-spots and spectra, and the conclusion I came to was that there is really no widening of the dark lines over the spots, but what you do get is the widening simply due to the fact that the plate is there less exposed.

Mr. Common then gave a description of a new form of mounting for a very large reflector.

Mr. Turner. I have a paper calling attention to some predictions which were made of the Moon's error in order to assist Capt. Grant in determining the boundary of Mashonaland. His longitude observations were necessarily rough, as they had to be completed on the spot. He wanted to know the approximate errors beforehand of the Moon. There is no important theoretical question raised, but the paper compares the prediction with the known errors observed at Greenwich since the prediction was made. I have, however a much more important paper, which I am sorry Mr. Thackeray is not here to read. It is an investigation which was entirely initiated by Mr. Thackeray, and my part in it is only that of helping to get it ready for to-day. It will be within the knowledge of Fellows that there has been great discussion about variations of latitude. There are statements by Mr. Chandler to the effect that there is a period of fourteen months, which is undergoing a slow change. I have talked with many astronomers, and I find that views, as regards this change in period, are certainly diverse. Prof. Newcomb accepts the period, but says it has not changed, as Mr. Chandler finds, in the last century or so. Prof. Young is quite ready to accept this variation. That is only more or less gossip. At Greenwich lately, Mr. Thackeray's attention was drawn to the question whether the Greenwich observations showed any change in latitude. Mr. Thackeray undertook an examination of the N.P.D. of four close circumpolar stars, and this is the most satisfactory material at Greenwich, and I think by means of two diagrams, which can be thrown upon the screen, you will see the evidence he finds for Mr. Chandler's period as given in his paper in the 'Astronomical Journal,' No. 267. Mr. Turner then explained the diagrams as shown on the screen.

Mr. Turner also exhibited some pictures taken during the longitude expedition from which he had recently returned.

The following papers were announced:—

C. E. Peek. "Observations of the Variable Star S Ursæ Majoris."

R. P. Sellors. "The Orbit of γ Coronæ Australis."

S. W. Burnham. "The Motion of ζ Cancri."

S. W. Burnham. "The New Star in Auriga."

J. E. Gore. "On the Orbit of Σ 2525."

H. H. Turner. "On the Systematic Errors of the Moon in Right Ascension."

J. T. Hendrick. "Probable Error of the Clock Correction when both the Clock Rate and the Instrumental Constants are found by a Least-square Solution of a Single Night's Obervation."

H. C. Russell. "Enlarged Star and Moon photographs."

Royal Observatory, Greenwich. "Observations of Brooks' Comet (*d* 1892) and Swift's Comet (*a* 1892)."

A. A. Common. "On the Best Mounting for a Large Reflector."

Rev. A. Freeman. "On the Sidereal Period of the New Satellite of Jupiter."

E. E. Barnard. "Nova Aurigæ."

E. E. Barnard. "Discovery of a Comet by Photography."

E. E. Barnard. "Note on the Period and Distance of the 5th Satellite of Jupiter."

James Gordon. "Corrections to Hansen's 'Tables de la Lune.'"

W. G. Thackeray and *H. H. Turner.* "On the Variation of Latitude as Indicated by Recent Observations at the Royal Observatory, Greenwich."

W. H. M. Christie. "On the Photographic Magnitude of Nova Aurigæ, as determined at the Royal Observatory, Greenwich."

Radcliffe Observatory, Oxford. "Estimations of Magnitude of Nova Aurigæ, made with the Barclay Equatorial."

Royal Observatory, Greenwich. "Observations of Holmes' Comet (*f* 1892)."

The following Candidates were proposed for election as Fellows of the Society:—

J. I. Berry, 20 Alwyne Road, Canonbury, N. (proposed by W. S. B. Woolhouse); *Martin Brendel*, Ph.D., Privat-Docent at the University, Greifswald, Germany (proposed by H. H. Turner); *E. G. Dixon*, Teacher, 78 Alderney Street, S.W. (proposed by John Newton); *H. D. Taylor*, Optical Manager to Messrs. T. Cooke & Sons, 20 Bootham Terrace, York (proposed from personal knowledge by Albert Taylor).

Rev. J. B. Brearley, M.A., F.R.G.S., St. James' Church, Oldham, Lancashire; *Herbert Hancock, M.A., F.R.Met.Soc.*, Bancroft's School, Woodford, Essex; *T. T. Knowles, M.A.*, Royal Grammar School, Lancaster; *A. T. Odell-Sorrell*, St. Mary's Grove, Chiswick, W.; *Edward Stroud*, Prisca Coborn Foundation School, Tredegar Square, E.; and *John Tatlock, Jun., M.A.*, New York, U.S.A., were duly elected Fellows of the Society.

Prof. W. L. Elkin, Yale College Observatory, New Haven, Conn., U.S.A.; *Prof. J. C. Kapteyn*, Observatory, Groningen, Holland; *Prof. Hugo Seeliger*, Observatory, Munich, Bavaria; and *Dr. Hermann Struve*, Observatory, Pulkowa, Russia, were duly elected Associates of the Society.

ROYAL METEOROLOGICAL SOCIETY.

The first Meeting of the Session was held on Wednesday evening, November 16th, at the Institution of Civil Engineers, 25 Great George Street, Westminster, Mr. A. Brewin, Vice-President, in the Chair.

Mr. E. T. Adams, Mr. A. L. Jones, M.R.C.S., Mr. J. E. Prince, and Mr. W. Tattersall, C.E., were elected Fellows of the Society.

An interesting paper by Mr. J. Lovel was read, on the "Thunderstorm, Cloudburst, and Flood at Langtoft, East Yorkshire, July 3rd, 1892." The author gives an account of the thunderstorm as experienced at Driffield on the evening of this day; the full force of the storm was, however, felt in the wold valleys which lie to the north and north-west of Driffield, where great quantities of soil and gravel were removed from the hillsides and carried to the lower districts, doing a large amount of damage. Many houses in the lower parts of Driffield were flooded, and a bridge considerably injured. The storm was most severe in a basin of valleys close to the village of Langtoft, where three trenches 68 yards in length and of great width and depth were scooped out of the solid rock by the force of the water from the cloudburst. From the appearance of the trenches, it is probable that there were three waterspouts moving abreast simultaneously. This particular locality seems to be favourable for the formation of cloudbursts, as there are records of great floods having previously occurred at Langton, notably on April 10th, 1657, June 1857, and June 9th, 1888. The author gives, in an appendix, a number of observations made on similar occurrences, together with particulars and opinions as to the cause of such outbursts by several eminent authorities.

Mr. W. H. Dines also read a paper: "Remarks on the Measurement of the Maximum Wind-pressure, and Description of a new Instrument for Indicating and Recording the Maximum." For some years the author has been conducting a large number of experiments with various forms of anemometer, and in the early part of the present year recommended the adoption of the tube anemometer for general use, as it appears to possess numerous advantages. The head is simple in construction, and so strong that it is practically indestructible by the most violent hurricane. The recording apparatus can be placed at any reasonable distance from the head, and the connecting-pipes may go round several sharp corners without harm. The power is conveyed from the head without loss by friction, and hence the instrument may be made sensitive to very low velocities without impairing its ability to resist the most severe gale. In the present paper the author describes an arrangement of this form of anemometer which he has devised for indicating very light winds as well as recording the maximum wind-pressure.

Some further Observations of the Fifth Satellite of Jupiter.

I HAVE given in the 'Astronomical Journal,' no. 275, a short account of the discovery of the Fifth Satellite of Jupiter, along with a set of micrometer measures of its position.

The satellite has been observed since on every available occasion, and a few facts, in addition to those already printed, have been obtained.

There seems to have been some error in the transmission of the telegram announcing the discovery. This telegram was written by Professor Holden on Sunday, September 11th, and was telephoned to San José by myself for transmission by telegraph.

The short interval between the observations had led me to assign an erroneous period of $12^h\ 36^m$. Through some error in the telegraph office this became $17^h\ 36^m$ and was so distributed throughout the world.

A later period of $11^h\ 50^m$ was also found to be erroneous.

As soon as a sufficient interval had elapsed, the periodic time was more closely approximated to by elongations of September 10th and October 21st, the latter being derived from measures on each side of elongation.

The period thus obtained was $11^h\ 57^m\ 20^s$.

From an elongation on October 28th the period is $11^h\ 57^m\ 17^s$.

The satellite has been observed on several occasions by Professor Young and his assistant Mr. Reed with the 23-in. at Princeton. Professor Stone has also observed and measured it with the 26-in. at the University of Va., and I just learn that Professor Hough has seen it with the 18½-in. at Evanston, Ills.

During the first observations—those published in the 'Astronomical Journal'—the satellite was faint from moonlight and poor seeing, and I was under the impression it could not be seen with anything less than 26 in. It has, however, been an easy object in the 36-in. under good atmospheric conditions, and has several times been well seen with Jupiter in the field, though in general to see it at all it is necessary to hide the planet by an occulting bar or by throwing it out of the field. When the seeing is not good the satellite is difficult from the diffused light in the field.

I have looked for it on two occasions with the 12-in., but have not yet seen it with that instrument.

Under the best conditions I have examined the satellite with a high power, but have not seen it other than as a star. Nor have I been able to catch the slightest glimpse of its shadow when in transit. From an estimation of its light, it has been provisionally assigned the 13th magnitude.

From these considerations, it is probable the satellite does not exceed 100 miles in diameter.

When the wind does not shake the telescope, measures of the position of this small object can be made with great exactness, by the method of illuminating the wires devised by Mr. Burnham, though when near the planet the wires can always be seen without illumination.

Under the best conditions the satellite can be followed to within 8″ of the planet's limb.

In all the later measures a piece of smoked mica has been used in front of the eye-lens to dull the dazzling light of Jupiter; the planet can then be readily seen at the same time with the satellite. The mica covers one half the field, as shown in the sketch.

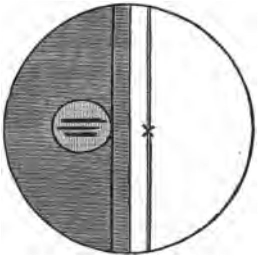

Diagram of field half-covered with smoked mica.

I have succeeded in getting a few observations at the west elongations; but these have been so unfavourably placed, that nearly all the observations of the satellite have been obtained east of the planet.

These few observations show that the west elongation distance is nearly 1″ shorter than the eastern. There is therefore a sensible eccentricity to the orbit.

The mean of ten measured east elongations between Sept. 10 and Oct. 28 is 48″·10 ± 0″·011, while the mean of four (including a discordant one) west of the planet gives 47″·42 ± 0″·042, the probable error of a single observation being ± 0″·11 and ± 0″·17. These are all reduced to distance 5·20.

These two values, which can scarcely be the extremes, represent a distance from the centre of Jupiter of 112,520 miles and 110,930 respectively.

The measures for latitude seem to show that the satellite's orbit lies sensibly in the plane of Jupiter's equator, which would signify that it is not a recent addition to the Jovian family.

Mt. Hamilton, 1892, Nov. 4. E. E. BARNARD.

Nomenclature of the Satellites of Jupiter.

GENTLEMEN,—

The discovery of a fifth satellite of Jupiter will, we may safely assume, raise again the question of the nomenclature of his satellites which has been allowed, to a great extent, to sleep. I have noticed, however, and regretted an increasing tendency to recognize the names for the four which were proposed by Mayr (commonly called Marius), but allowed to drop lest their acceptance should seem to imply that of his claim to have made their discovery before Galilei. One would have thought that the giving two of these names to small planets (Europa and Io, discovered by Goldschmidt at Paris, and C. H. F. Peters at Hamilton College, Clinton, on the 6th of February, 1858, and the 19th of September, 1865, respectively) would have finally established their rejection as applied to other bodies in the solar system. Yet more than one astronomical book has lately attempted to revive them. Hitherto there has been no real occasion for using special names, for as the satellites were all discovered about the same time, the designations I., II., III., IV., according to their respective distances (proceeding outwards) from Jupiter, have been amply sufficient. But the discovery of a fifth somewhat alters the matter.

Now it is the undoubted right of a discoverer to name the body or place he has discovered; but this right, like others, appears to have certain recognized limitations, the principal one being that the name selected should be not only appropriate but of a similar kind to those of other bodies similarly circumstanced. Thus neither the name proposed by Herschel for the planet discovered by him (the first discovery of a primary planet ever made) was allowed to stand, nor his own name which some endeavoured to substitute for it.

All the planets bore mythological designations, and therefore it was thought that the new one must have a name of that kind, and the suggestion of Uranus was, when made, very soon generally accepted. When Piazzi discovered the first of the small planets he proposed to call it Ceres Ferdinandea. The first fell in with the rule and was accepted, the second was dropped. Not only was it superfluous as well as inappropriate, but it offended, like Herschel's Georgium Sidus*, against another canon which had been tacitly accepted. This was that a heavenly body should not be called after an earthly patron, who ought to be content with having the place at which, or the instrument with which, it is discovered called after him. Thus the "Lick telescope" or the "Lick observatory" are appropriate terms in every sense, and no one would dream of objecting to them. But I apprehend that if Prof. Barnard should suggest (I do not mean to hint that I think it likely) to call the interior satellite of Jupiter by the name Lick, the idea would be rejected, and even scouted, without hesitation. The same feeling, doubtless, operated against adopting Galilei's proposal to call the four old satellites of Jupiter the Medicean stars; still more against that of calling each individually after members of Cosmo's family.

Coming to more modern times, we find that a suggestion of what may be called a semi-mythological character for the satellites of Mars was adopted by the discoverer and at once generally accepted. Would it not be possible to do something of the kind in the present case?

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* It must have surprised many, like myself, that Prof. Newcomb should have rendered this "Star of the Georges" ('Popular Astronomy,' 2nd edition, p. 365). Not only will not the Latin admit of it, but it is well known that Herschel intended to compliment only one of the Georges, his patron George III.

As I have said, Mayr's names of the four Galilean satellites cannot be accepted, and two of them have been for some time otherwise appropriated. Saturn's satellites have the mythological names proposed by Sir John Herschel. But would it not be better, in this case, to follow rather the analogy of the satellites of Mars? Deimos and Phobos are stated, in mythical poetry, to have been the names of the attendants who harnessed the steeds of Mars, or rather Arēs. With what should we especially associate Jupiter in a similar way? Surely with the thunderbolt. My suggestion, then, offered in all diffidence, is that the Galilean satellites be still called, as formerly, by their numbers I., II., III., IV. (now used in so many books that they have almost become names in this connexion), and that the new interior one be designated Fulmen, or (if a Greek form be preferred, as in the case of the satellites of Mars) Keraunos. Yours faithfully,

Blackheath, 1892, Oct. 10. W. T. LYNN.

THE following letter from Prof. Young will doubtless be of general interest:—

MY DEAR MR. TURNER,—

I was very glad to get the newspaper clipping you sent me, and so to learn of your safe return to Greenwich. But by this time you will have seen the more recent numbers of the Astron. Journal, with the *corrected* period of the new satellite ($11^h\ 57^m{\cdot}3$). This of course makes it certain that what you and Mr. Reed saw on Sept. 26th could not have been the satellite, since that night it did not come to E. Elongation until about $19^h\ 15^m$ G.M.T. or $2^h\ 15^m$ Princeton time. At the time you were looking, it must have been most of the time behind the planet.

We were put off the track entirely by the erroneous period, and Mr. Reed did not succeed in finding the satellite until Oct. 10th, when he found it at E. Elongation about 18^h G.M.T., within an hour of the time when it ought to have been at *Western* Elongation according to the $11^h\ 50^m$ period.

He has observed it a number of times since then, on Oct. 11th, 13th, 28th, and 29th, but has not succeeded in getting any really satisfactory set of measures on each side of an elongation to fix its time accurately. He got a few measures before elongation on Oct. 10th and one set after it on the 29th, and the rough elongation times deduced from these, combined with Barnard's Elongations on Sept. 10, 11, 12, and 14, give $11^h\ 57^m{\cdot}38 \pm 0^m{\cdot}10$ as the period.

I sent a summary of them to the A. J. a day or two ago, and I presume you will see the note soon.

I have not been to the Observatory at all since you were here; but I am much better now and am planning to go down to-night, for the first time. Have had a real siege with sciatica in the left leg.

Please remember me most respectfully to Mr. Christie, and believe me
Yours very truly,
Princeton, N. J., 1892, Nov. 8.
C. A. Young.

Ephemeris of Jupiter's Fifth Satellite.

The times of the greatest eastern elongations, Oct. 21, $17^h\ 2^m{\cdot}8$, and Oct. 23, $16^h\ 49^m{\cdot}0$ G.M.T., which Prof. Barnard has computed from his measures before and after elongation, are respectively $5^m{\cdot}6$ and $2^m{\cdot}7$ later than the corresponding times derived from the data on which the ephemeris on page 272 of the 'English Mechanic' of Nov. 11 depends. Till the detailed measures become known, or till later observations are available, some doubt about the right correction cannot be removed. In preparing the following ephemeris, I have adopted $722^{\circ}{\cdot}64$ as the daily rate of the satellite's motion in its orbit, or $11^h\ 57^m{\cdot}37$ as the period of revolution. The position-angle P+90° of the planet's equatorial diameter is added, as the distances $x = a \sin(l - \mathrm{L})$ of the satellite from the polar axis ought to be measured in this direction.

Greenwich Noon.	P+90°.	a.	b.	l−L.	Diff.	G.M.T. of greatest elong. West.		G.M.T. of greatest elong. East.	
	°	"	"	°	°	h	m	h	m
1892 Dec. 1...	65·65	57·95	+1·48	47·58	1445·22	7	23	13	22
3...	·64	57·59	1·46	52·80	·20	7	13	13	12
5...	·64	57·23	1·45	58·00	·19	7	2	13	1
7...	·63	56·87	1·43	63·19	·17	6	52	12	51
9...	·63	56·51	1·41	68·36	·15	6	42	12	41
11...	65·63	56·14	+1·39	73·51	·14	6	32	12	30
13...	·63	55·78	1·36	78·65	·12	6	21	12	20
15...	·64	55·41	1·34	83·77	·11	6	11	12	10
17...	·64	54·04	1·31	88·88	·09	6	1	12	0
19...	·65	54·65	1·28	93·97	·08	5	51	11	50
21...	65·66	54·31	+1·26	99·05	·06	5	41	11	39
23...	·67	53·95	1·23	104·11	·04	5	31	11	29
25...	·68	53·59	1·20	109·15	·04	5	21	11	19
27...	·69	53·23	1·17	114·19	·02	5	11	11	9
29...	·71	52·87	1·15	119·21	1445·00	5	1	10	59
31...	65·73	52·52	+1·12	124·21	1444·99	4	51	10	49
1893 Jan. 2...	·75	52·17	1·09	129·20	·98	4	41	10	39
4...	·77	51·82	1·07	134·18	·97	4	31	10	29
6...	·79	51·48	1·04	139·15	·96	4	21	10	20
8...	·82	51·14	1·02	144·11	·95	4	11	10	10
10...	65·84	50·81	+0·99	149·06	·93	4	1	10	0
12...	·87	50·48	0·97	153·99	·92	3	51	9	50
14...	·90	50·16	0·95	158·91	·92	3	41	9	40
16...	·93	49·85	0·93	163·83	·90	3	32	9	30
18...	65·97	49·54	0·91	168·73	·89	3	22	9	21
20...	66·00	49·23	+0·89	173·62	·88	3	12	9	11
22...	·04	48·93	0·87	178·50	·88	3	2	9	1
24...	·08	48·64	0·85	183·38	·87	2	53	8	51
26...	·12	48·35	0·84	188·25	1444·85	2	43	8	42
28...	66·16	48·07	0·82	193·10		2	33	8	32

Markree Observatory,
Collooney, Ireland.
A. Marth.

Plato.

As the announcement of an apparent change upon the floor of this formation seems to have excited some interest, it may be well to supplement my former remarks by a brief account of our more recent researches. Mr. Elger suggests that the dates upon which the former observations were made should be given. They were as follows:—July 14, 15, 16, 17, and 18, 1891, February 8 and 14, and March 12, 1892. The corresponding dates of the New Moon were July 5, January 29, and February 27. It will thus be seen that the formation was viewed upon the first, second, third, fourth, fifth, seventh, and tenth days after the Sun rose upon its interior. As Mr. Williams remarks, the interior detail appears much more conspicuous under a full illumination than it does either earlier or later. By studying it carefully at this time many more spots have been detected not mentioned by the Committee. A number of these are doubtless included among those seen by Mr. Williams. It should be stated that the preceding end of the crater has not been studied under quite such favourable conditions as the following end. It may be mentioned in passing that on the evening of September 30 we detected the new streak Z^2, described by Mr. Williams in the August number of 'The Observatory.' It extended from the neighbourhood of 14 about halfway across the floor, and was fairly conspicuous. As Mr. Sprague suggests in a recent communication, my number 40 is probably identical with the crater shown by Neison in his drawing of Plato soon after sunrise.

A careful study of Plato was made this last September, and the positions of the more conspicuous angles of the floor and of the following craters were determined by the micrometer:—1, 2, 3, 16, 17, 30, 40, 41, 42, 46, and 47. Numbers 3 and 17 were selected as primary stations, and the position-angle and distance of each of the other craters was determined from them. The results were plotted the next day upon paper, four intersections being obtained upon each crater. Where any discordance appeared the measurements were repeated. The observations were made upon September 5 and 7. Ten more or less discordant angles were repeated, and comparing the results after correcting for libration, the average deviation from the mean upon the two nights was found to be $1^{\circ}\cdot9$. This comparison included one error of exactly 10°, which may have been due to an erroneous scale-reading. Six discordant distances were also repeated, and omitting one of them, evidently due to an erroneous reading, the average deviation from the mean was found to be $0''\cdot08$. It will therefore be seen that the errors in the positions of these craters upon the chart are not likely to be much greater than those which would naturally be introduced in the process of transferring the measured angles and distances to paper. The remaining craters and bright points were located from drawings. A comparison of this chart with that of the Committee will show considerable discrepancies between them, and it is possible that my numbers 40, 41, 42, 43, 44, and 46 are identical with the numbers 25, 6, 12, 18, 13, and 19 of the Committee. As other identifications, however, are possible, it seemed on the whole better to employ the new numbers rather than run the risk of having the same number apply to different craters in the two charts. This question could probably be settled by a reference to the original drawings, if they are still accessible.

Observations upon Plato were secured upon every night that it could be satisfactorily seen throughout the lunation, and all the brighter craters and points visible were arranged in the order of their visibility upon each night. The numbers of the most conspicuous craters are given first. Where several were nearly equally well seen they are enclosed between dashes.

1st day, August 30.—Ninth day after new Moon. The floor of Plato was first rendered visible by a narrow streak of light illuminating the craters numbers 3 and 30. Later the floor became half uncovered, and crater number 1 became partly visible. Order of visibility: 17, 3, 1,—30,—11, 32, 55,—10, 20,—41,—4.

2nd day, August 31.—1,—17, 3,—30, 40,—11, 9,—32, 20, 41,—42, 4.

3rd day, September 1.—1,—3, 17,—30,—40,—9, 11.

4th day, September 2.—1,—3, 17,—30,—40,—9,—11,—2,—47, —32, 20,—41,—4.

5th day, September 3.—1,—3, 17,—30.

6th day, September 4.—1,—17,—30, 3,—2, 47, 9, 40,—11, 41, 20,—32, 51, 54, 46, 44, 42, 53, 4, 43, 10,—48, 45, 4,—9.

7th day, September 5.—1,—17,—30,—3,—2, 9, 47, 40,—11, 20, 41, 32,—42, 46, 51, 16,—53, 28, 48, 44, 49, 43, 50,—4, 10, 26, 7,—54, 34, 8, 31, 56,—29, 33, 14,—53, 52, 58, 59, 57, 45. Numbers 0, 15, 21, 22, 23, and 35 could not be found, although a careful search was made for them.

8th day, September 6.—1,—17,—30,—3,—20,—32,—2,—9, —11,—47,—40,—41,—53,—42.

9th day, September 7.—1,—17,—30,—3,—9,—11,—40,—32, —20,—2,—47,—41.

10th day, September 8.—1,—17,—30,—3,—9,—11,—20,—32, —2,—40,—47,—7.

11th day, September 9.—1,—17,—30,—3,—9,—2,—20,—40, —11,—32,—47,—7.

12th day, September 10.—1,—17,—30,—3.

The two remaining days of visibility while not completely cloudy were sufficiently so to render observations of the finer details practically valueless.

Taking up the different craters in order, we see that number 1 is undoubtedly the most conspicuous one at all times, and that 17 is usually ranked as second. Before full Moon, 3 is generally ranked third, and 30 fourth; but after full Moon their order is reversed. This is due to the fact that the interior of 30 is unusually brilliant, while the interior of 3 is only of the ordinary brightness. Crater number 40, which possibly is identical with number 25 of the Committee, was invisible upon the first day of the observations on account of the curvature of the Moon's surface. Upon the next three days it was the fifth in order of visibility, after that becoming fainter, but never very faint. At the next lunation upon September 30, it appeared large and well defined, and is recorded "as large and well marked as 30." This crater is an interesting one, because in the Report of the Committee ten actual craters are mentioned and six probable ones; but 25 is not among them. It is included among the bright points that "had not presented themselves as craters." From this statement it will be seen that it is a matter of some interest to determine whether the present crater really coincides with number 25 of the Committee, or whether it is distinct from it as was at first assumed.

Numbers 9 and 11 are always well seen, and seem subject to no important variation. Number 2 emerged from the shadow of the outer wall upon the fourth day, and at once became an important crater. Under favourable seeing it was upon two days ranked as fifth in order of visibility. Numbers 32 and 20 are surrounded by white patches, which render them relatively conspicuous when the seeing is poor or with a small telescope, while under more favourable circumstances they are comparatively insignificant. Number 47 is an interesting crater. It emerged from the shadow with number 2 upon the fourth day, and by the sixth was placed sixth in order of visibility. It has a large and well-defined central crater. The observations of last year in regard to its importance are therefore confirmed. It does not appear at all among the thirty-six objects visible to the Committee. Numbers 7 and 31 were both detected at this lunation. They are both faint, the latter extremely so, and was only seen upon one date, under full illumination. No craters could be seen in them, and indeed in the case of 31 the matter is entirely out of the question, since the point itself is so difficult of observation. In the Report of the Committee both are stated to have well-defined craters.

My last year's observations upon number 4 have been further confirmed. The whole of the bright region in which it is situated is slightly elevated, and slopes gently towards the west. As a bright point $1''\cdot 2$ in diameter it was best seen under low illumination, being ranked eleventh in order of visibility upon August 30, while by September 5 it had sunk to the twenty-fourth place. It would be a very difficult matter at present, even here, to determine its position micrometrically, yet it was one of the four craters selected for measurement by the Committee. It has never yet been seen by me as a crater, yet the Committee rank it with 1, 3, and 17, as craters generally visible, and Mr. Williams states that it was clearly seen by him, as a crater, as late as October,

1888. It would be interesting to determine if this was the last time that it was seen under that form.

Among miscellaneous observations may be mentioned some upon the diameters and elevations of the walls of the craterlets. Measurements made upon the night of August 31 with a power of 700, give No. 1 a diameter of 5000 feet, and Nos. 17 and 3, 4000 feet. Adopting the altitude of Plato δ given by Neison of 6369 feet, a comparison was made on August 30 of the length of its shadow with the outside shadow of number 30. Two practically coincident measurements made the length of this shadow $1''\cdot 2$, corresponding to an altitude of 150 feet. Crater number 1 appears to reach the same elevation. Upon August 30 at 16^h G.M.T. the illumination upon the floor of Plato reached within 10 miles of number 25, yet the crater itself was not visible. This would indicate that its altitude must have been less than 250 feet. Measurements made upon August 31 of the shadow cast inside the crater of number 1 give it a depth of 530 feet, assuming the bottom to be flat. In the same way the depths of numbers 17 and 3 were found to be 400 feet each.

In closing this paper I may say that as this region seems to be one in which there is reasonably good evidence of actual physical change having occurred within recent times, I have devoted considerable time to its study, and have endeavoured to render our knowledge of its present features as complete as it is practicable for one observer to make them with our modern means of research. The total number of points located is forty-four. It is not likely that an error of $1''$ has been made in the location of any of the larger craters, and this is amply sufficient for purposes of identification in the future. It is certain that at present there is no crater existing upon the floor as conspicuous as the fifteen that are indicated upon the chart by circles, that has not been observed. It is probable that the thirty most conspicuous craters have been observed. It is certain that the fifteen craters indicated by circles all have central craters. W. H. Pickering.

Arequipa, Peru, 1892, Oct. 15.

Making Black-and-White Lantern-slides.

The manufacture of the ordinary lantern-slide where delicate and soft graduations of light and shade in monochrome are required is an art that can be learned with a moderate amount of patience. But when exactly the opposite condition of things is in demand, a slide that will give hard black-and-white contrasts, as, for instance, that required to show distinctly a pen-and-ink drawing on the screen, or a copy of a line engraving such as that of a spectrum chart, the manipulation required to produce such a slide has been up to the present one of considerable difficulty, and a process which will at once yield good results has been a want much felt.

The faults usually met with are two in number—either the high lights which should be represented by absolutely clear glass have an annoyingly opaque haze over them although the lines may appear sufficiently black; or the high lights are thoroughly clear and transparent, but the lines faint, ofttimes affected with a loss of continuity, certainly illdefined and of a pale brown colour. In both the extreme cases I have mentioned the results are particularly objectionable; but what is more to the point, the diagram cannot be seen at any distance from the screen, and the lettering is often conspicuous by its absence. On account of the difficulty, then, in obtaining these special class of slides with commercial dry plates, it struck me it would be worth while to carry out the necessary experiments to render modern methods applicable to modern requirements. As I am now in a position to speak positively upon the subject, and in response to the kind invitation of the Editors of this Journal, I thought it would be advantageous, seeing the Council of the Royal Astronomical Society have provided so elaborate a projection apparatus, to publish a method I have found most suitable, in the hope it may assist those desirous of projecting their diagrams by lime-light during the reading of their papers, and who may not care, or have the technical knowledge sufficient, to worry out a process for themselves.

The subject may be divided into two heads—obtaining a suitable negative and making the slide.

Obtaining the Negative.—The selection of a dry plate is, I know, very much of a personal matter, but the one I find most suitable for the purpose is called "The photo-mechanical plate" and manufactured by Mawson and Swan, 33 Soho Square. It has been made from the first with this class of work in view. It has but one disadvantage—it requires about eight times the exposure of an ordinary extra-rapid one. When time is of consequence very good results can be obtained by using Paget's xxxxx times plate or the Isochromatic by Edwards, but as each of these will in most cases require subsequently intensifying (which is not the case with the photo-mechanical when correctly exposed) it is a matter of doubt whether any real saving of time is effected. The negative must possess exactly the reverse of what is required and aimed at in obtaining a view or a portrait—it must be hard, exceedingly dense, and possess not a trace of half-tones; yet the portions of the film corresponding to the lines of the drawing must be very clear and free from fog. It is just this property which makes the use of the photo-mechanical plate of such a distinct advantage, for it can be most fully exposed and violently developed without the lines getting fogged: hence the high lights can be allowed to become exceedingly dense. Expose fully* and for development make the two following solutions :—

Sol. 1.		*Sol.* 2.	
Hydrokinone	160 grs.	Sodium Hydrate ..	160 grs.
Sod. Sulphite	2 oz.	Water	20 oz.
Acid Citric	60 grs.		
Pot. Bromide	40 grs.		
Water to	20 oz.		

Take six drachms of each and four of water †; this will be sufficient for two plates in one dish. Develop until the film is as black as pitch (the lines, however, will be capable of being seen even when the plate is so much developed if held for a moment before the ruby glass), wash, fix, and rewash for three hours and dry in the usual manner.

If not dense enough, by which is meant that the high lights are rather transparent, the negative must be intensified, but if the exposure has been sufficient and the developer be good, such should not be necessary.

Note.—Paget's or Edwards's plates *will* choke in the parts corresponding to the lines long before the intense blackness required in the high lights can be obtained, hence the development must be discontinued before that takes place and resort had to the following method of intensification.

Prepare two solutions:

Sol. 1.		*Sol.* 2.	
Mercuric Chloride..	100 grs.	Nitrate of Silver....	10 grs.
Pot. Bromide	100 grs.	Water	10 oz.
Water	10 oz.		

After the silver is dissolved in No. 2 solution (keep from intense light whilst it is so doing) add, whilst vigorously stirring, cyanide of pot. until the precipitate immediately formed is nearly dissolved —say one third of a tea-spoonful in the bottom of the mixing-jar.

Note.—If too much cyanide is put in at first and the precipitate is quite dissolved, re-add some more crystals of silver nitrate until more precipitate is deposited and sufficient not to disappear even with renewed stirring.

Re-wet the negative, place in a Tyler's plate-holder and immerse in a plentiful quantity of No. 1 solution, which, it should be stated, can be returned to the stock-bottle for after-use, a remark that also applies to solution 2. Watch the *under* side of plate until the film is quite as white as paper, then remove and wash for at least three hours to avoid stains.

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* As a guide to the time necessary, a diagram required 5 minutes exposure at $\frac{F}{45}$ in a good light (not direct sunshine) when Watkins' exposure meter required 9 seconds to equalize the paper with the standard. This was in November.

† In cold winter weather omit two drachms of the water.

Note.—The longer the film is acted on by this chemical the greater the final intensification.

After washing, place in a plentiful quantity of No. 2 solution, watching under surface until it becomes quite black, then wash for two or three hours and dry in the ordinary manner. A plate-holder is *strongly* advocated for this process, as the solutions are *exceedingly poisonous* and the silver solution dirties the hands most deeply, stains which seem to resist any treatment save perhaps rubbing with a crystal of either cyanide or iodide of potassium.

Note.—Markings in the film always appear to me to arise from insufficient washing. If the negative is not densely black in the high lights it can be put through the process again and again if necessary; but it must be distinctly understood that if the photo-mechanical plates are used with sufficient exposure, no intensification should be required at all. Too much intensification can be reduced by using a solution of hypo (say 40 grains to the ounce), but care must be taken, as it will remove the whole of the chemical action in a very short space of time *.

Making the Slide.—There is little difficulty in doing this when the negative is of the right quality. Personally for this purpose I always prefer the new rapid lantern-plate for giving black tones only, issued by the Paget Prize Plate Company. Five seconds exposure is usually enough. Use the same developer and the same proportions as for negatives, but add two grains of pot: bromid. or a trifle over in each case. It is convenient to have a separate stock-solution of the hydrokinone with the addition of 60 grains of bromide all ready mixed. Develop until the lines are jet-black and lift out of the solution to soak for about half a minute, after which place the slide in water and then in the hypo, or do this directly the high lights begin to appear a little fogged, it will clear in the fixing-bath. E. J. Spitta.

Two Large Telescopes.

[Continued from p. 392.]

The best use to put such an enormous telescope as that already mentioned would probably be to devote it to a general survey of the most important objects; for this purpose its great focal length and light-grasping power would be very valuable, with an alt-azimuth mounting and a fairly approximate movement in the two planes an object might be kept in the field well enough for continuous eye-work, though not, perhaps, accurately enough for very delicate measurements.

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* Full details of this intensifying process, which I believe was designed by Monckhoven, will be found in Capt. Abney's "Instruction in Photography."

Even if something more than the parallactic movement of the equatorial had to be given up, the great gain that results from the altazimuth form of mounting as regards the difficulty of properly and effectually providing for the comfort and safety of the observer would more than compensate for it.

Sir William Herschel's 4-foot telescope was mounted in a very effectual manner too well known to make a description necessary; it has served for a model of several other reflectors which have done admirable work, as, for instance, the telescope mounted on this plan used by Sir John Herschel in his survey of the southern heavens.

The observer, however, is in the open air and at a considerable height when the telescope is pointed to the zenith. The tube of the telescope is necessarily a close one, and the whole of the mounting is exposed to the wind and weather. This form of mounting is very well suited for making sweeps of a degree or two in altitude, allowing the objects to come into the field of view for observation without movement of the telescope in azimuth; but it does not lend itself to the contiuuous observation of an object, nor is the position of the observer a very good one.

The plan used by Lord Rosse in mounting the 6-foot telescope restrained the movement in azimuth to a comparatively short distance on each side of the meridian. The arrangements for the observer are slightly better for continuous work on an object within the range of the telescope in right ascension, enabling the study of nebulæ to be carried on in a better way than the mounting of Sir William Herschel's telescope permitted.

Mr. Lassell's four-foot telescope was mounted equatorially; but an inspection of the drawing of it and the observing tower is enough to show that the limit of size had almost been reached for that plan—a remark that also applies to the 4-foot reflector at the Paris Observatory. The earlier telescopes made by Sir William Herschel and, I believe, one made by Sir Isaac Newton were mounted in a way that kept the eye-end at a fixed height. Miss Caroline Herschel's comet-sweeper was mounted in this way; but instead of moving the whole of the framework carrying the telescope in azimuth, Miss Herschel's telescope and the arrangements supporting it in altitude moved round on a central pivot. I do not know who was the maker of this particular form of mounting; it is certainly a most admirable form for the purpose for which it was made.

This plan of having the centre of motion in altitude close to the eye-end renders the position of the observer invariable as regards his height from the ground; his movement in following the telescope in azimuth is determined by the radius of the eye-end sweep, which in cases under consideration would be about half the focal length of the telescope, a very small matter with a small telescope.

The largest telescope I know of mounted on this plan is that made by Dr. Draper, and so well described in his work on the construction of a 15½-inch silver-on-glass telescope, published by the Smithsonian Institution. Here we have a framework roughly in the form of a square, with sides equal in length to the telescope; at one upper corner of the square the telescope-tube is swung in trunnions, one of which, being hollow, allows the light from the mirror to come to a focus just outside the trunnion—the telescope-tube being counterpoised by weights working at the end of bars supported at the diagonally opposite corner of the square to that where the eyepiece is situated, and connected to the mirror-end of the tube by wire ropes in such a way that their action is exactly equal and opposite to the weight of the end of the tube under varying angles, thus permitting the mirror-end of the tube to be raised or lowered while the eye-end remains at the same height.

In the case of Draper's telescope, the square framework carrying the tube and counterpoise weights was supported on a central pivot, and so the whole could be moved round as required. This, with the required inclination of the telescope-tube, allowed the telescope to be pointed to any celestial object without altering the height of the eyepiece, exactly as in Miss Herschel's telescope.

The observer was provided with a movable platform, which was quite independent of the framework carrying the telescope and could be moved to follow the eye-end of the telescope as required. The same part of the mirror is always uppermost in this form, and that makes the edge support much more easy than in the case of an equatorial, where the mirror may have any part of the edge uppermost.

There are many points about this form of mounting that are very good; one of the most important perhaps is the absence of heavy and costly constructional work. A very large mounting could be made very cheaply, using ordinary wrought-iron latticework girders and covering with thin sheet-iron—much the best material for surrounding a reflecting telescope. If it were decided to mount a reflector of the size given, I do not know any form that might be exactly copied so well as the mounting made by Dr. Draper. There is nothing impossible; but the large movement of the eye-end would be a source of inconvenience, as the platform would have to have a motion of varying amount to keep the observer within reach. In order to get rid of this trouble the important modification I would propose is to shift the pivot, which, as we know, is central in Dr. Draper's mounting, to a point exactly under the trunnions, balancing the framework by counterpoise weights. This could be very easily done by having a large *caisson* under, floating in water—the exact movement being kept correct by a strong central pin. We would have in this arrangement a framework similar to that of Dr. Draper, but turning on a pin or centre under the trunnions instead of under the centre of framework; the sweep of the eye-end would then be reduced from about 70 ft. (that is, about ½ the focal length) to about 7 ft. (that is,

½ the diameter of the tube *plus* a little for the projection of the trunnions). This framework with an open lattice-work tube with counterpoise arrangement would form the telescope proper, in place of a tube two lattice-girders on edge braced together at the top and bottom would connect the mirror and the trunnions, about the trunnions and for a little distance above and below there might be a tube connected by some flexible material to the portion of the framework near the trunnions, and the whole of the rest of the framework might then be covered in so as to form a large dark chamber in which the mirror would be free to work under the best conditions as regards freedom from currents of air; this would also form the best kind of workshop in which the mirror could be made, by having the machine for working it just under the end of tube when vertical, so that it could readily be taken off machine and attached to tube or taken off for silvering.

With such a great amount of surface the effect of the wind might be injurious as regards definition, and it might be best to have, covering the whole of what we might call the mounting proper, another framework running on outer rails and covered over so as to protect the mounting within. This outer framework would then surround the mounting, being made large enough to allow it to rotate in azimuth some 30°, so that the outer framework would only require movement at intervals of two or more hours. At the upper portion, where the tube of the telescope would project, the outer framework would of course be cut away to allow the telescope to move without touching it—a platform being built to come up close to the trunnion forming the eye-end and forming part of a chamber which could be of any desired size on the outer framework, access by any suitable means being provided on this outer framework. The movement of the trunnion with the eye-end, being in a circle of about 7 ft. radius, would not be very great during the movement of the telescope during the two or more hours that it could be used without the movement of the outer framework.

This arrangement of outer and inner framework becomes very large, but it would answer admirably; nearly one half becomes useless as regards sheltering the telescope and might be to a large extent dispensed with, the only objection being the great power the wind would have upon such a large surface rotating in one corner. This could be largely prevented by making the sides with considerable slope and having an arrangement for anchoring to the rails if necessary. As regards the actual amount of material required in the construction of the covering, this would be largely reduced. The inner chamber formed by covering the inner framework and forming the telescope might be obtained, not separately as proposed, but by using the outer framework and cover. We should then have a telescope of the following description:—An open framework in general construction very similar to that designed by Dr. Draper in all the important parts, excepting that the centre of rotation would be under the trunnions instead of under the centre of framework; this would have to be balanced, but with a large floating circular vessel there would not be any difficulty in doing that. This framework telescope would be covered by a strong outer framework of the shape on plan of a triangle, the apex being the centre of rotation of the inner frame and the base the line that the outer portion of this framework would sweep—say an angle of about 40° or 45°, so that the inner framework would be free to sweep through this angle without coming in contact with the outer one. The junction of the tube of the telescope near the trunnions with the outer frame could be made airtight by any flexible material that would not communicate the vibrations of the outer framework to the telescope. The platform for the observer and his dark chamber &c. could be of almost any size.

The mounting here suggested would enable a constant stream of visitors, if needed, to go up and down the outer framework without in any way interfering with the telescope: it would permit the mirrors to work under the best conditions, and, above all, it would allow the observer to work under the most favourable conditions, not only as regards his personal safety and comfort, but as regards the absence of that constant movement that is needed with the equatorial. In fact, a little consideration will show that, apart from the question of making a show telescope, this arrangement is really the best that could be used for a large reflector, which might be called an exploring telescope, and be used for many purposes and in many situations where the ordinary mounting could not be taken, owing to difficulties of transport.

As regards the mirror, although the increase from 5 to 10 feet is great, there ought not to be any great difficulty in casting and properly annealing the glass; that done, the rest is only a matter of time, care, and patience. There is not the least difficulty beyond the making of the glass that need cause any doubt as to the ultimate success. From the thickness found to be perfectly sufficient in the 5-ft. mirror, a thickness of from 10 to 12 inches for a 10-ft. would be ample.

Even if the idea of making this large telescope be abandoned, I hope that a similar one may be made on the lines I have suggested, as for a given amount of outlay it seems likely to give the best results, particularly in that general exploring work for which it is so peculiarly suited. A. A. Common.

Discovery of a New Comet in Andromeda.

We have had very little decent weather for observing in London of late, but I make a practice of never missing an opportunity. Sunday, November 6th, was far from clear, and did not improve as the night deepened, so that after looking at Jupiter and a few double stars and finding the seeing unsatisfactory, I made up my mind to close observing at 11.30 P.M.

Ever since the temporary star appeared in M 31, I have taken an observation of that nebula at every chance I have had in case any further outbreak took place, and on Sunday night it occurred to me to try whether I could see the small *comes* to μ Andromedæ under such conditions. On swinging the telescope round from β I caught something nebulous in finder and mistook it for M 31. Going to the eyepiece of reflector I recognized at once that it was not the nebula and called out involuntarily, "What is the matter? there is something strange here." My wife heard me and thought something had happened to the instrument, and came to see. I recognized at once that it was a new comet, but before I could get more than a rough position clouds hid it. I said to my wife: "This is coming end on, and will be a big fellow, and I must get a position before I leave it if possible." I wrote at once to Mr. Maunder, Mr. Maw, and Mr. Kidd, of Bramley, and posted, and then got clearer sky, and with Slade micrometer made it immediately preceding Σ 72, the interval of passing the centre being $1^m\ 10^s$ at a single trial.

Having only restricted sky room, I was unable to make a second measure or see if motion was perceptible. I measured the diameter of the nebulosity as exactly five of the one minute divisions of the micrometer. As these are calculated for a 6-feet focus, and my focus is 78½ inches, the estimated diameter is rather over than under the mark, but I consider the milky state of the sky somewhat reduced apparent diameter, and that probably 5′ is exact. The position in declination is very close also, as both comet and star travelled accurately on wire. As regards R.A., the difference did not exceed $1^m\ 10^s$ I am sure, but it may have been 1^s less.

On Monday morning I wrote the Astronomer Royal, but as I omitted to say I saw the Andromeda nebula at the same time, he very naturally at first thought I had blundered. My friend Mr. Kidd, with whom I have corresponded for years, and to whom I am indebted for much counsel and kindness, also suspected a mistake, but on Monday evening he saw the comet with the naked eye, as did also Mr. Bartlett of Bramley. I wrote again to Greenwich, and being now satisfied of the reality of the discovery, they took measures to spread necessary information. Mr. Maw accepted the matter at once and wrote to congratulate me.

It is unnecessary for me to enter into any particulars of further observations because they are sure to be furnished by those with better and more exact appliances, at any rate as regards position; but perhaps my impressions of change, when seen a second time on November 14th, at 10.45, may have some value, because no one but myself and Mrs. Holmes appears to have seen it for some days after its first appearance with any telescopic means. On the 14th the nucleus was much less distinct and much less bright, the boundary of the nebulosity was much less well defined and less perfectly circular and far more like a nebula. The intrinsic brilliancy was less, but the diameter largely increased. Only getting a look at it betwixt clouds and showers, I was not able to measure, but by comparison with the half-field of Slade micrometer I estimated the diameter at from 8′ to 9′.

It will be a pleasure to learn that others have had better weather than myself, and gathered a large number of facts for our instruction. EDWIN HOLMES.

Selenographical Notes.

BRIGHT STREAK SYSTEMS VISIBLE UPON THE FULL MOON.—A recent issue of the 'Astronomische Nachrichten' contains a very suggestive paper by Prof. Pickering in which hitherto received opinions as to the length, width, and general character of these mysterious features are controverted and an hypothesis advanced to account for their origin and development which is very ingenious and novel. As results of these lunar investigations, which were made with the 13-in. telescope in the phenomenally clear and steady atmosphere of Arequipa, it was found—

(1) That the streaks round Tycho do not radiate from the apparent centre of this formation, but point towards a multitude of minute craterlets on its south-eastern or northern rims. Similar craterlets occur on the rims of other great craters, forming streak-centres.

(2) Generally speaking, a very minute and brilliant crater is located at the end of the streak nearest the radiant-point, the streak spreading out and becoming fainter towards the other end. The great majority of lunar streaks appear to issue from one or more of these minute craters, which rarely exceed a mile in diameter.

(3) The streaks which do not issue from minute craters usually lie upon or across ridges, or in other similar exposed situations, sometimes apparently coming through notches in the mountain walls.

(4) Many of the Copernicus streaks start from craterlets within the rim, flow up the inside and down the outside of the walls. Kepler includes two such craterlets, but here the flow seems to have been more uniform over the edges of the whole crater, and is not distinctly divided up into separate streams.

(5) Though there are similar craterlets within Tycho, the streaks from them do not extend far beyond the walls of this formation. All the conspicuous streaks about Tycho originate outside its walls.

(6) The streak-systems of Copernicus, Kepler, and Aristarchus are greyish in colour and much less white than that associated with Tycho; some white lines extending south-east from Aristarchus

do not apparently belong to the streak-system. In the case of craterlets lying between Aristarchus and Copernicus, the streaks point away from the latter.

(7) There are no very long streaks. They vary from ten to fifty miles in length and are rarely more than a quarter of a mile in width at the crater. From this point they gradually widen out and become fainter. Their width, however, at the end farthest from the crater is seldom more than five miles.

The latter statement, regarding the length of the streaks, is so utterly opposed to current notions, that Prof. Pickering goes on to exemplify it by a reference, among others, to the too well-known parallel rays extending from the north-east of Tycho to the region east of Bullialdus. Careful examination shows that these streaks originate from a system of minute craters lying from 30 to 60 miles beyond Tycho. He says:—"They then enter a couple of broad slightly depressed valleys. In these valleys are found numerous minute craters of the kind above described, with intensely brilliant interiors. When the streaks issuing from those craters near Tycho are nearly exhausted, they are reinforced by streaks from other craters which they encounter upon the way, the streaks becoming more pronounced at these points. These streaks are again reinforced further out. These parallel rays must therefore not be considered as two streaks, but as two series of streaks, the components of which are placed end to end."

Hence it would appear that what have hitherto been regarded as continuous bright streaks are really made up of a succession of comparatively short lengths, diminishing in brilliancy but increasing in width till they reach the next crater lying in their direction, when they are reinforced. and the same process of gradual diminution of brightness and subsequent reinforcement goes on from one end of the streak to the other.

The appearances recorded were observed under the exceptionally favourable atmospheric conditions which usually prevail at Arequipa, with a magnification ranging from 450 to 1120 diameters on the 13-in. telescope. Prof. Pickering remarks that the confirmation of his observations can only be effected by means of instruments of the first class, used under the best circumstances. The craterlets he speaks of are rarely more conspicuous than that which stands within Linné, and unless this is easily distinguished it is vain to look for them, and, presumably, it is equally hopeless to attempt to detect the peculiarities relating to the varying breadth and brilliancy of the bright streaks.

The following explanation is briefly suggested to account for the origin of the rays:—The Earth and her satellite may differ not so much as regards volcanic action as in the densities of their atmospheres. "Thus, if the craterlets on the rim of Tycho were constantly giving out large quantities of gas or steam, which in other regions was being constantly absorbed or condensed, we should have a wind uniformly blowing away from that summit in all directions. Should other summits in its vicinity occasionally give out gases mixed with any fine white powder, such as pumice, this powder would be carried away from Tycho, forming streaks."

Assuming the existence of these regions of condensation and of pumice-emitting craters subjected to the currents of the resulting wind, it is easy to conceive that the effect would be a more or less linear deposition of the white material, which would become thinner the farther it extended from the source of eruption; but in the case of the Tycho rays, which, whether they are continuous (as they certainly appear to be in ordinary telescopes) or whether, as Prof. Pickering asserts, they consist of a series of comparatively short streaks placed end to end, gradually becoming fainter in their course till they are reinforced by the *ejecta* from fresh craterlets, the fact remains that they appear to radiate, as the late Prof. Phillips, the geologist, remarked, "like false meridians, or like meridians true to an earlier pole of rotation." We may ask, Why do these active craters happen to lie upon these particular radiating lines? for there are no indications of such a general drift of white material as we may suppose would have been manifested had the pumice-ejecting craters been less definitely arranged.

This is a consideration bringing us back to the Nasmyth hypothesis, which attributes the radiating streaks to the presence of cracks caused by the action of an upheaving force, and explains their light colour by supposing an outwelling of lava from them which has spread for some distance on either side. If the lunar surface has been fractured in this fashion, we can easily imagine that craters might be developed upon these fissures, which, being in communication with the interior, would continue to eject pumice, or white matter of some kind, long after the general surface and other types of craters, large and small, had attained a stable condition.

The confused network of streaks round Copernicus seems to respond more happily to the requirements of Prof. Pickering's hypothesis: for here there is an absence of that definiteness of direction so manifestly displayed in the case of the numerous Tycho rays, and we can very well comprehend that with an area of condensation surrounding this magnificent ring-plain, beyond the limits of the streaks, and a number of active craterlets on its rim and flanks, the white material ejected might be drawn outwards in all directions by wind-currents and, settling down, assume forms very similar in appearance to the complicated arrangement of light streaks which we see.

As there is no branch of Selenography which has been so neglected as the bright streak-systems, these recent investigations are the more welcome. It is to be hoped, despite the somewhat discouraging remarks of Prof. Pickering relating to the optical and atmospherical requirements necessary for the confirmation of his observations, that the attempt will be made by many both in this country and elsewhere who, though they are not located in the pure transparent atmosphere of Arequipa, possess instruments which will show craterlets on Tycho's rim and in other situations quite as small as one mile in diameter or even less, and which ought also to enable them to trace the course of the light streaks with their remarkable reinforcements and degradations of brightness.

Space will not allow a detailed reference to the phenomena presented by other ray-centres, or to the delicate streaks observed within Plato, Archimedes, Fracastorius, and similar formations, in which the relation between the streaks and the spots and crater-cones standing upon these light areas is too palpable and direct to be a result of chance; but it appears to me that the new hypothesis affords a more satisfactory explanation of this type of streak than it does of the far-extending Tycho rays.

Beaumont House, Shakespeare Road, THOS. GWYN ELGER.
Bedford, 1892, Nov. 19.

Some Longitude Trips.

[Continued from p. 405.]

PARIS looked in 1892 very much as in 1888. The gentleman with the gold lace cap who paces backwards and forwards on the steps of the Grand Hotel was slightly greyer, but just as impressively courteous as when in 1888 he requested me not to smoke a pipe in the sacred precincts of the quadrangle. I wonder whence comes this widespread antipathy to a pipe? In the St. Lawrence Hall at Montreal a notice is posted in the reading-room, "Smoking absolutely prohibited, particularly pipes." So that a pipe is not only outside the pale of the respectable, but even ultra-absolute. There is, however, a good excuse for the authorities at the Grand Hotel, for excellent cigars can be obtained close by. I suppose the Grand Hotel is more or less of a home for most Englishmen in Paris. During my stay at Montsouris, where our insular prejudices are not thoroughly understood, it was my daily solace to journey Grand-Hotel-wards for breakfast. Not only is the cuisine good, but casual strangers in the billiard-room are often very good company. Once I met a man who had been in five railway accidents, and yet was deliberately choosing the journey by rail to Brindisi on his way back to Australia. He explained that his experiences (which were connected with his profession) had not made him exceptionally cautious about railway travelling, but about dangerous places; and first and foremost of hazards he put the journey down the English Channel.

The weather was very bad for the first week in Paris, and thus I was enabled to see many new things. At the Paris Observatory there was M. Loewy's new Great Coudé. This instrument represents the integrated experience obtained by the construction of several other instruments; and though the modifications introduced are matters of detail, those who have had experience in devising new instruments know well how much labour M. Loewy must have expended in attaining the perfection apparent in the present instance. The easy working of the whole instrument from the eye-end is very striking, and indeed almost alarming to the stranger. One wants the nerves of a railway signalman, accustomed to move big things at a distance, to feel indifferent when turning a little handle and knowing that a great object-glass is swinging about outside. M. Loewy has, of course, long ago acquired this indifference, for he has been a persistent observer since his invention of the coudé, which makes it possible to observe in a warm room under comfortable conditions, although previously consideration for his health had compelled him to renounce personal work with the telescope.

The Director, alas! I did not see. It was during my stay in Paris that he died, and I had only the melancholy satisfaction of being one of the many who assembled to do him the last honours. One had almost forgotten that Admiral Mouchez was a sailor as well as an astronomer and geodesist; but Havre had not forgotten his gallant defence of her in 1870, and her representatives were there to recite their eulogy in company with the men of science. Under the guidance of Commandant Defforges I paid a visit to the French Academy and found that M. d'Abbadie, the President, who received me most kindly, can speak English better than most of us, being in fact an Irishman; and I heard the beautiful French in which M. Cornu expounds the investigations of those who may be lucky enough to have him for expositor. And then there was a meeting of the Bureau des Longitudes—a friendly chat of men of science round a table, to which I was fortunate enough to be invited in company with Prof. Newcomb, who was on his way to the Dublin Tercentenary Festival. He had something of interest to say about the variation of latitude, for he does not believe in the change of period claimed by Mr. Chandler; but this is too large a question to deal with here. Then, again, Commandant Defforges took me to Meudon and to Breteuil. M. Janssen had been in Paris that morning and journeyed with us to Meudon. On the way he showed us the wooden structure which was to be placed on Mont Blanc as an observatory. It will be remembered that attempts made last year to reach solid rock by tunnelling through the ice were unsuccessful; but before abandoning the summit for the winter a wooden hut was placed on the snow with two levels attached, designed to test its stability. An officer ascended in February last and found that the levels showed no signs of disturbance; and it was concluded that possibly a wooden structure well sunk in the snow would be permanent enough to allow of residence sufficiently long for special investigations. Accordingly, carpenters were set to work to build a pyramidal tower, affording excellent temporary accommodation for two or three people, and weighing, I think, about 10,000 kilos.

I have not heard whether it was successfully raised to the summit, but the attempt was to be made this summer.

Meudon itself is not only astronomically but historically interesting. It is the ruins of an Imperial palace burnt by the Prussians in 1870. M. Janssen's long tubes for studying the spectrum of compressed oxygen run the length of the Imperial stables, and the stalls for the Imperial horses are now become merely a number of very efficient supports for these tubes; or, in one or two cases, a stall has been made a laboratory for a particular investigation. But M. Janssen has recently been presented with a large equatorial, which is being splendidly mounted and housed. The dome is some distance from his dwelling in the palace, and there is accordingly provided in an annexe to the dome a couch where the tired observer may rest till morning.

At Breteuil is the Bureau des Poids et Mesures, and it would be difficult to find any other institution where there is so much to be seen and admired, and so able a cicerone as M. Benoit, the director. The general principles of measuring and weighing are well known, but the details of the operations must be seen to be appreciated. M. Benoit was personally at work on an optical method which he had devised for measuring the linear expansion of small pieces of metal. Plane faces are carefully polished on the metals and placed in contact with glass surfaces, to form Newton's rings; and expansion of the metal with increasing temperature causes a dilatation of the rings, which can be measured with the greatest nicety.

The various refinements introduced into the comparison of measuring-bars with standards are too numerous to describe here. There is a tolerably complete collection of measuring-bars at Breteuil just now, for all the old bars are being re-compared, with a view to the co-ordination of all the French survey-work from early times; and on the other hand there were the modern standard metres with their elaborate form of cross section. But perhaps the weighing-room was the most novel spectacle for a stranger. In the four corners of a large room are four delicate balances, three of which are somewhat of the familiar form. The place for the operator, however (after the first approximate weighing is made, and the balance has been left to itself overnight), is in the centre of the room, and long handles stretch thence to the balances, by means of which he is enabled to perform all the requisite operations, including interchange of weights, from a distance. The fourth balance is in an air-tight case, and yet the same operations are performed, the weighing being thus conducted in an approximate vacuum; and the long handles are here a little more elaborate, being furnished with a miniature working-model, on the principle of the pantagraph, of what is going on inside the case. After seeing even one tenth of what M. Benoit has to show one cannot but realize the fundamental importance of the work under his charge, and rejoice that the English subscription to this

International undertaking was not withdrawn as was threatened a few years ago.

During the fourth and last " set of three fine nights " of the Paris longitude, for which Mr. Hollis crossed to Paris and I returned to Greenwich, Prof. McLeod arrived from Montreal, and we entered upon a preliminary discussion of the results obtained in the spring. Two conclusions of importance were deduced. First the signals across the Atlantic were found to be practically as consistent as those over the land lines, though the time of transmission was of course longer, being about $0^s{\cdot}25$ (one way) instead of a few hundredths merely. The errors introduced by the signals may in fact be neglected compared with those of time determination, excepting only the error of the assumption that the times of transmission in opposite directions over the cable are the same. There is no way of avoiding some such assumption unless several cables be employed, which would greatly increase the cost of the operations: but the limit of error thus introduced may readily be estimated; for supposing the time of transmission in one direction to be double that in the other (which would seem an extreme supposition), the error in longitude introduced by the above assumption would be one sixth of the whole time for the double journey, or about $0^s{\cdot}08$ for the cable in question.

Secondly, Prof. McLeod's time determinations at Montreal combined with the known Greenwich clock errors and signals gave very consistent values for the longitude of Montreal not differing seriously from the adopted value. The reduction must, however, be regarded as quite preliminary.

Bad weather prevented our making a good determination of personal equation in the short time available before my start for Canada on August 5; but it is hoped that there is sufficient material of other kinds to make this unimportant.

All who cross the Atlantic for the first time, no doubt feel vaguely and infinitesimally the old anxiety of Columbus, and experience in the same degree his relief at sight of land. It is also an impressive thing to see your first iceberg, especially if he be a big fellow with great blue caves washed out of him. But such matters are an old story to those who have crossed and will not be appreciated by those who have not until they do. Concerning Montreal, however, where I arrived after a most enjoyable voyage, it is fitting that some details should be recorded; for the aim of our operations is to endow this city with a new importance. But lest any charge of prejudice should be made, I will perform this task vicariously, by copying from a book. Mr. Robert Barr has lately written on the subject, and says :—" It is rather a curious thing, when you come to think of it, that there is but one perfect city in the world. Of course everybody will understand at once that I refer to Montreal. For either a summer or winter visit it offers more attractions than any other place on earth. The noblest river in America flows past its doors with water as clear as crystal, but for fear that the Londoner should miss the muddy Thames and be homesick, Montreal has arranged at immense expense that the turbulent and dusky Ottawa should, at this point, empty into the transparent St. Lawrence. To make the Englishman feel perfectly at home, Montreal is so thoughtful as to be an island, and also possesses a mountain the view from which far excels that from the Rigi. The largest railway (and best managed) in the world terminates at Montreal, the longest bridge in all creation crosses the river at that point, the biggest hotel in Canada is there, as well as one of the grandest Cathedrals in the New World—with a lift, too . . The Montreal man is a good all-round specimen of humanity, an athlete, temperate, resolute and hardy, a boon companion and a staunch friend; an Englishman with modern improvements. . . . Tickets for Montreal may be had at all reliable agencies. Don't take a return; you will want to stay there" *.

After this deposition I need only briefly refer to my personal experiences. During my first stroll through the city I was greatly struck by the forest of telegraph poles, and very nearly by a large lump of ice which a man threw out of a cart on to the side walk. It is the Transatlantic way of delivering ice. From the moment the stranger registers at the hotel above referred to, he is apparently free of the place, without any subsequent inquiries as to his name or number, even at meal times, when most excellent repasts are provided for those who have time to wait. This system is certainly pleasant, and very possibly profitable, for the losses sustained from carelessness or dishonesty are probably much less (in Montreal) than the additional expenses which would be incurred by any check system.

One great advantage of the hotel was its proximity to the M^cGill University, where was the observing station. The growth of this modern University has been very rapid, and I wish I had the ability or even space to give some adequate account of it. The most striking and most modern feature is a huge building, due to the lavish generosity of Mr. W. C. MacDonald of Montreal, combined with the wonderful energy and enterprise of Prof. H. T. Bovey, wherein a student may learn Applied Science; that is to say, everything which a healthy young man with his way to make in the world might possibly require to know. A casual inspection of the building might give the impression that carpentering and mechanical work were to be substituted for the "book-learning" of the older Universities, but this is not the case. They are added to the curriculum as important features in education, not necessarily for application in after life, but as a guarantee of a hand that is not all thumbs; but theoretical training is still the groundwork of the course. The faculty over which Prof. Bovey presides has, for instance, its own Professor of Mathematics, and does not depend on the crumbs which might fall from the table of the Faculty of Arts.

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* 'The Idler,' September 1892, p. 223.

But my business was with the Observatory. The benefactor who will endow Astronomy at the M^{c}Gill University on the same scale as the more "applied" sciences is yet to arise; though there is an admirable little equipment for time-service and a small equatorial. Prof. M^{c}Leod is Professor of Astronomy and Geodesy, and could not therefore have chosen a more suitable field for the exercise of what energy is left him from teaching than such determinations of fundamental longitudes as the present, where Astronomy and Geodesy meet. But we may all wish him without hesitation a better equipment and a larger staff, for there is no better place to pitch a good observatory than that where a capable Director pre-exists.

Some alterations in the wooden building where my transit was to be mounted (for, as before remarked, each observer carried his instrument with him) were not completed on my arrival, and there were, in fact, no shutters for the roof; but the difficulties introduced were trifling, and the free circulation of air was an advantage, for the weather was very hot. It was rather a severe change to come from a cold sea-voyage to a climate where sitting still to register transits at any time of night, divested of as much clothing as decency would allow, was an uncomfortably warm exercise. But the definition was extremely good throughout—no tremulous effects of heat; each star a steady round little disk with two or three rings. The climate, as far as my experience of two weeks in August goes, is an additional reason for a more ambitious observatory at Montreal. H. H. TURNER.

[To be continued.]

CORRESPONDENCE.

To the Editors of 'The Observatory.'

The present Comets.

GENTLEMEN,—

My best view of comet Holmes was on the 16th. As seen in my 4¼-inch refractor, it had a very peculiar appearance, being a bright disk, round or very nearly so, 12′ in diameter, with very little variation in brightness, rapidly fading at the *np* edge, though more gradually at the *sf*. In the middle was a strip of brighter light, whose brightest part, forming an indefinite nucleus, was considerably *np* the centre of the circular disk; I suspected other points of concentration of light in it. This strip formed the core, as it were, of a tail, the rest of which was very faint; both the core and the faint part reached to 20′ from the nucleus at a position-angle of 140°, *i. e.* nearly *sf*; the width of the tail being equal to the diameter of the head. The appearance of the bright strip was similar, in miniature, to that of an ordinary naked-eye comet with a long narrow tail.

Comet Brooks at 17ʰ 45ᵐ G.M.T. on the 16th was plainly visible to the naked eye, with a straight tail 2° 35′ long.

It is erroneously stated on p. 415 that Swift's comet is beyond the reach of ordinary instruments, for it continues a conspicuous object in a 4¼-inch refractor, and is fading only very slowly. On the 31st ult. it still had two faint tails, pointing in opposite directions, *p* and *f*; and the extension of the head towards *n* was decidedly less than towards *s*. The total light of the comet was equal to a star of the 11th mag.

Denning's comet on the 25th ult., though fainter than Swift's, was also visible with my telescope. Yours faithfully,

Sunderland, 1892, Nov. 23. T. W. Backhouse.

Seven Comets Visible.

Gentlemen,—

It may be noted, as a very unusual circumstance, that no less than 7 comets are now visible in telescopes of about 10 inches aperture. The following is a list of these comets arranged in the order of Right Ascension:—

Letter, 1892.	Discoverer.	Date of Discovery, 1892.	Date of Perihelion, 1892.	Approx. place, Nov. 25, 1892. α.	δ.
				h m	° ′
f..	Holmes	Nov. 6	April 19	0 41	+36 43
c..	Pons-Winnecke	Mar. 18	June 30	0 51	−16 58
b..	Denning	Mar. 18	May 11	5 18	− 9 28
d..	Brooks	Aug. 27	Dec. 28	11 4	−10 51
g..	Brooks	Nov. 21	..	13 5	+15 30
e..	Barnard	Oct. 12	Aug. 26	21 28	− 1 8
a..	Swift........	Mar. 6	April 6	23 47	+32 18

b and *c* are *very* faint, *a* and *e* are faint. The date of perihelion of *f* is doubtful. Elements for *g* have not yet been published.

Yours faithfully,

Bristol, 1892, Nov. 25. W. F. Denning.

Regarding the Announcement of Professor Barnard's Discovery of a Fifth Satellite to Jupiter.

Gentlemen,—

Announcements of discoveries in America are sent to the Harvard College Observatory for transmission abroad. Harvard College is the designated central station for the United States, as Greenwich is for England.

On September 11 a telegram announcing Professor Barnard's discovery of a fifth satellite to Jupiter was duly sent (by him) at 1 P.M., local time, to Harvard College Observatory and also to the press. The telegram gave the time of east elongation Sept. 10. This is the datum missing from the dispatch which you quote from the 'Standard' newspaper in the 'Observatory,' No. 193. The desired information was not "withheld." I have no knowledge as to why the telegram did not reach England in the form in which it was sent from here. I regret that any failure occurred. We are, however, in no wise responsible. Professor Barnard has communicated a paper to the Royal Astronomical Society, giving an account of his discovery, to which I beg to refer you for details.

Yours faithfully,
EDWARD S. HOLDEN.

University of California,
Mount Hamilton, 1892, Oct. 25.

OBSERVATORIES.

COLUMBIA COLLEGE, NEW YORK.—Mr. H. Jacoby has published another valuable series of astro-photographic measures. "The group of stars surrounding β Cygni was frequently photographed by Rutherfurd. Six of the plates were carefully measured under his direction with his improved micrometric apparatus provided with a glass scale for measuring distances." The ultimate result is an accurate catalogue of 42 stars of the 8th and 9th magnitudes for 1875·0.

PUBLICATIONS.

THE CAPE ASTRONOMERS AND THEIR WORK.—A small pamphlet published by Murray and St. Leger of Cape Town (reprinted from the 'Cape Times') is well worth some trouble in acquiring. It is a lecture delivered by Dr. Gill at Kimberley on Oct. 5 last, and is an admirable and compendious history of the Cape Observatory. We should have liked to give some extracts, but it seems invidious to make any selection. Perhaps, however, it may be well to draw attention to the following appeal:—"On the site occupied by Herschel's telescope stands a granite obelisk, sent out by the scientists of England to mark the place where such great contributions to human knowledge had been made, and yet how comparatively few persons there are in the Colony who even know of its existence. The very name exists no longer, and Feldhausen, where the founder of your education system lived and laboured, now goes by the trumpery and meaningless title of 'The Grove.' Change that name now back to Feldhausen, which is, and ever will be, a sacred one in the annals of science, and which ought to be ever dear to you. Further, let some large-hearted men or women who have benefited by the progress of the Colony, found, on that conditional return to the old name, a Herschel scholarship in the Diocesan College which now flourishes in the house where Herschel lived, and thus something will be done to redeem the neglect in the past, and that something of a character which would be most grateful to the family of him whose memory ought to be revered in South Africa. If this lecture bear no other fruit, I shall be well satisfied if it happily be the means of removing the reproach here indicated."

Double Stars.—The Memoirs of the R.A.S., Vol. L., contain some double-star measures which we are glad to welcome. Mr. Maw is evidently one of those men who, after a busy day, can find rest at the eye-end of a 6-inch telescope. The telescope must be a good one, for we notice that some stars measured are quite on the limit of divisibility with a 6-inch. There are 500 measures of about 160 stars which have been selected with care, and each measure is of value. Is OΣ 18 too far south? We know of no measure since Hall in 1886. And may we ask Mr. Maw to add the following to his working list?—OΣ 50, 53, 82?, 92, 112?, 160, 216, 229, 237, 266, and 296. Some will be further tests for his instrument and—himself. By the way, Mr. Maw is mistaken on one point: there *are* about six other measures published of OΣ 149, and one by Hall (just published) is 1891·14, 288°·5 –0″·66. T. L.

NOTES.

Comet Notes.—Brooks' Comet (*d* 1892) is getting too far south for convenient observation in these latitudes; we, however, append an ephemeris for December:—

	R.A.	S. Decl.		R.A.	S. Decl.
1892.	h m s	° ′		h m s	° ′
Dec. 1 ..	11 33 44	16 50	Dec. 17 ..	13 1 16	30 53
5 ..	11 54 51	20 46	21 ..	13 23 58	33 31
9 ..	12 16 34	24 27	25 ..	13 46 40	35 46
13 ..	12 38 45	27 51	29 ..	14 9 12	37 37

The brightness reaches a maximum on Dec. 12, when it is 28 times that at discovery.

Herr Schulof has deduced elliptical elements for the comet discovered by Barnard on Oct. 12; these show so close an agreement to Wolf's Comet of last year that he concludes with some probability that the two comets had a common origin.

The following is an ephemeris for December:—

	R.A.	S. Decl.		R.A.	S. Decl.
	h m s	° ′		h m s	° ′
Dec. 5 ..	22 4 55	3 14	Dec. 17 ..	22 44 53	4 26
9 ..	22 18 12	3 53	21 ..	22 58 14	4 55
13 ..	22 31 32	4 26	25 ..	23 11 30	5 36

The brightness on Dec. 25 is ¾ of that at discovery.

Mr. Barnard has given an account of the discovery of this comet. On Oct. 12 a long exposure photograph was taken of the Milky Way in the neighbourhood of *α* Aquilæ, on developing which a nebulous trail was noticed, which was at once conjectured to be caused by a comet; the region was too near the horizon for examination that evening, but on the following day the object was quickly picked up, nearly 2° from its place the night before.

In another column Mr. Holmes gives an account of the discovery of his comet on Nov. 6. We congratulate him on his success and hope that it will stir up English amateurs to more systematic work in a direction in which, with the brilliant exception of Mr. Denning, they have hitherto been somewhat remiss. It is rather disappointing that the comet has passed perihelion some months ago (different computers get results varying from April to August) and is now rapidly receding from both the Sun and Earth. It is strange to find that two months or so before the discovery it must have been a conspicuous naked-eye object, although it does not seem to have been observed anywhere. Mr. Schooling, of Hammersmith, took a photograph on Oct. 18 on which the comet appears plainly. A position is now being deduced from this photograph, which will be of value in correcting the elements of the orbit. Prof. Kreutz, from observations on Nov. 9, 13, 17, deduces an elliptical orbit with a period of 7 years and concludes a recent close approach to Jupiter. The interval between the observations is too short for this result to be entitled to much confidence. The following approximate ephemeris for the first half of December is by Berberich:—

	R.A.			N. Decl.			R.A.			N. Decl.	
	h	m	s	°	′		h	m	s	°	′
Dec. 4	0	41	45	35	54	Dec. 12	0	43	46	35	17
8	0	42	35	35	35	16	0	45	17	35	1

His ephemeris makes the brightness on Dec. 16 $\frac{4}{9}$ of that at discovery; other ephemerides make the diminution in light still greater.

A bright comet (*g* 1892) was discovered by Brooks on Nov. 21; this is the fifth comet of the year entitled to the epithet 'bright.' At discovery its place was R.A. $12^h\ 59^m\ 15^s.6$, N. Decl. $13°\ 50'\ 27''$; daily motion $1^m\ 32^s$ east, 25′ north.

THE VARIATION OF LATITUDE.—No. 277 of the 'Astronomical Journal' contains paper No. VII. of the series by Mr. S. C. Chandler on this subject. We hope to give some account of the whole series when it is complete; but meanwhile we may say that a wonderful diagram accompanying this paper No. VII. is worth looking at for a few moments by those even who are only remotely interested in the subject. It gives in diagrammatic form the whole of the evidence for the variation, and the law assigned to it by Mr. Chandler; and the accordance of this law with the observations of nearly 50 years is very striking.

THE MOTION OF THE SOLAR SYSTEM.—Prof. Porter has reduced 1340 proper motions contained in publication No. 12 of the Cincinnati Observatory. He has adopted Dr. Stumpe's method of dividing the stars into four groups, according to the magnitude of their proper motions; and his results are as follows:—

Group.	No. of Stars.	Annual P.M.	Sun's Way.		$\frac{c}{\rho}$
I. ...	576	<0″·30	281°·9	+53°·7	0″·16
II. ...	533	0″·30 to 0″·60	280 ·7	+40 ·1	0 ·30
III. ...	142	0″·60 to 1″·20	285 ·2	+34 ·0	0 ·55
IV.	70	>1″·20	277 ·0	+34 ·9	1 ·66

The quantity $\frac{c}{\rho}$ represents the angular motion of the Sun as seen from the mean star in the group; and it will be seen that Dr. Stumpe's result is confirmed, viz. that the proper motion of a star is an index of its distance from us; for the mean parallax of the group is directly proportional to the quantity $\frac{c}{\rho}$.

THE occultations of Mars and Jupiter by the Moon on September 3 and September 9 were well observed at the Lick Observatory by Prof. Barnard. "When passing behind the Moon a narrow shadow-band was noticed fringing the Moon's limb where it cut the planet," in the case of Jupiter but not of Mars. This is often seen at occultations of Jupiter, and it is perhaps due to contrast alone.

AMONG the lectures advertised for the present Session of the Liverpool Sunday Society, we notice one by Mrs. R. A. Proctor on the Life and Death of Worlds (Dec. 18), and one by Mr. W. E. Plummer on the Bidston Observatory, of which he has recently taken charge (February 26).

NOVA AURIGÆ is apparently again increasing in brightness according to an observation on November 29; but confirmation is required.

THE index for the present volume will be distributed next month.

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